HUMAN RESOURCES RESEARCH COUNCIL

Social Science and Social Policy

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Foreword

Can the social sciences contribute significantly to social progress? How close a relationship can be expected between social science research and social policy development? If there are links between the world of the social scientist and the world of the policy-maker, are they useful links, or are they mere bureaucratic forms without depth of meaning? Do policy-makers and social scientists have mutual goals, or is the intuitive, non-scientific, action-oriented world of the policy-maker too far removed from the rational, theory-oriented world of the social scientist?

As a research and development agency, the Human Resources Research Council of Alberta must concern itself with these questions. Research is significant and justifiable only if it is, ultimately, useful. This is particularly true of social science research, which deals with people, but which can, without direction, become curiosity-centered and too fragmented and inconclusive to be relevant.

This fragmentation, which inhibits the application of social science research is due, in part at least, to the tendency of the social scientist to reject grand, interrelated theories and for each investigator to work within his own discipline, using his own methods and data, with little attention to the perspectives and principles of other, often similar, disciplines.

While such an academic, "ivory tower" approach is not characteristic of all social scientists or even of most social scientists, it is common enough and traditional enough to have created in many policy-makers an attitude of intolerance and impatience with "egg heads." This is understandable, since policy-makers live a day-to-day existence, focussed upon immediate rather than long-range concerns. Unlike the social scientist, who may take the attitude that time is not important, the policy-maker or politician knows that time is important, although his consciousness of immediate needs may narrow his view and his interpretation of problems, causing him to confuse the symptoms and the cause. Further, his priorities are different. He must face the reality of justifying his decisions in order to stay in office.

These two perspectives are not irreconcilable. They are, in fact, reconciled in many instances. Many social science researchers are politically aware and realistic. Many policy-makers and politicians have developed a high level of insight into social problems. These instances of successful collaboration and mutual understanding may be regarded as guideposts in the continuing effort to establish a workable and satisfactory relationship between social science research and social policy.

The purpose of this conference was not to formulate such a relationship. Nor was it necessarily to arrive at consensus or fixed conclusions. Rather, it was to discuss the issue of how social science research can be applied to social policy, and through discussion to develop new insights into the issue. Two of the four papers gave theoretical and somewhat abstract views with practical overtones (Nettler and Paquet). The other two are discussions with concrete and specific suggestions (McQueen and Matejko).

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Human Resources Research Council of Alberta
Edmonton, Canada

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Editors' Note

Included in this volume are the four major presentations to a two-day conference-seminar on social science research and social policy, sponsored by the Human Resources Research Council of Alberta in February, 1971. The meetings were organized by Dr. Karol Krotki, Professor of Sociology at The University of Alberta and Coordinator of Socio-Economic Opportunity Studies at the Human Resources Research Council, assisted by Susan McDaniel.

Contributors to this book include Dr. Gwynn Nettler, Professor of Sociology at The University of Alberta; Dr. David McQueen, Professor of Economics at Glendon College, University of Toronto; Dr. Alexander Matejko, Associate Professor of Sociology at The University of Alberta; and Dr. Gilles Paquet, Professor of Economics at Carleton University. The papers by Dr. Nettler, Dr. McQueen, and Dr. Matejko were presented to the conference on February 19. Dr. Paquet's paper formed the basis for a seminar on February 20.

Although only the major presentations are included here, each of the three conference papers was discussed by a scholar knowledgeable in the appropriate field. Discussants for the Nettler, McQueen, and Matejko papers were, respectively, Dr. Lee Bean of the Population Council, Dr. Simon Goldberg of the Dominion Bureau of Statistics, and Dr. James Short, Professor of Sociology at Washington State University. At the dinner ending the conference, the speaker was Dr. Otto Larsen, Professor of Sociology at the University of Washington and Commissioner on the United States Commission on Obscenity and Pornography.

Knowing and Doing

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FEBRUARY, 1971

Knowing and Doing

Knowing may be defined as a kind of doing, but, whatever "knowing" refers to, it is not, in dictionaries and common parlance, the same as any action or all action. In ordinary lexicons, knowing is distinguishable from most doing. It is assumed that there are criteria by which the knowledge activity may be marked off from those other behaviors and human conditions in which we have an interest.

This preamble is required because there are philosophers who continue to contend that knowing cannot be segregated from doing and being. These thinkers maintain that what we are, where we are, and what we do so strikingly condition anything that might be separated out of us and called "knowledge" as to make impossible any dispassionate and impersonal statement of fact or probability.[1]

A popular version of this blending of knowing with being dilutes knowledge with the more agreeable solution of "perspective," "orientation," or "image." While there may be utility in considering all intellectual tools as fictions,[2] no one is able to act consistently upon such a premise.[3] At some juncture, at most daily junctures, our behavior assumes that our perceptions are not merely imaginings and that a real world, resistant to any dream, is "out there."

To substitute "perspective" for knowledge is to dissolve reality in point of view and information in interpretation. The logical end of such an anti-intellectual stance is silence, for, if perception of the world is nothing but a construction of it, if reality is nothing but a mirage, then no one can speak sensibly of consequences. No one can urge, rationally, that one decision is better than another and it becomes impossible to talk, in a referential way,[4] of being dead or alive, here or there. Such "social" terms as "birth rate," "income," "crime" become only definitions, pictures in the head, and there is certainly no point in a serious effort to deal with airier abstractions like "equality," "opportunity," or "justice." More to our present concern, such a phenomenalistic attitude toward the world makes it unnecessary to know anything before acting and reduces research to a trivial exercise, irrelevant to the more efficient satisfaction of desires.

The current fashion that "sociologizes" knowledge regards statements of "what-may-happen-if . . ." as fairly tested when one has identified who said it and whose interest the saying serves. This intellectual vogue condones the genetic fallacy and is blind to its error.

Against these popular denials of knowledge, some of them sponsored in the name of "social science," "common sense realism" postulates something to be known, "independent of the knowing mind."[5] (Lewis, 1929:193). It holds that there are events external to us to be identified, more or less well, and that the signs of *knowing* can be specified and publicly verified. The signs refer to conditions and happenings that do not move freely about with the social position of the inquirer. Thus, a cancer *is* whether one construes it as disease or defective thought. Similarly, rich men and poor can reasonably agree on a tally of dollars on the table before them.

A realist position allows that it is one thing to observe the association of social position with attention, evaluation, and belief. It is another matter to deduce from such correlation that reality is nothing but social construction.[6]

A realist position acknowledges that, while knowing and doing are separable, men frequently know their worlds in order to act upon them. This says nothing, however, about the quality of the action taken with differing degrees of information. It does not, for example, specify that the action for reason of which one tries to know must be aggressive rather than submissive, interventionist rather than escapist, or directed toward changing the world rather than our attitude toward it. Further, knowing in its varied styles need not indicate whether the type of action to follow upon a knowledge shall be individual (what I alone do), co-active (what I do with some others), or persuasive (what I urge others to do).

It will not serve here to define knowledge as so tightly embraced by action that knowledge *determines* the act. Such a definition would be just that, a tautology. Knowing *would be* doing, and *vice versa*, as some radical sociologists wish us to believe.

The reason for continuing to speak of action and knowledge as though they were separate systems is precisely that the kinds of thought accredited as knowledge are in actuality only loosely related to human action. This "irrelevance" applies, *a fortiori*, to human interaction.

The slippage between knowing and doing is assured by at least two facts. The first is that knowledge is seldom certain (although some philosophers might wish to reserve this honorific for those rules that are: Lewis, 1929:27). A second guarantor of a separation between knowledge and action is the fact that values other than any that might be influenced by a knowing are always (?)[7] involved in an intentional doing. We do not act upon *this* knowledge to get *that* end without the choice of

means itself being constrained by values other than the target. It has been noted, and will be repeated, that a would-be societal engineering is frustrated, not only by deficiencies in our knowledge, but also by our desires.

In sum, the anti-intellectual definitions that blur the distinction between knowing and other kinds of doing comfort the confusion of several meanings of the verb "to know," meanings that the Romance languages and, better yet, the Chinese vocabulary, have long distinguished. "To have knowledge" is not the same as to have information, or to be familiar with, or to be aware of, or to exhibit a competence. Knowledge of need need not be acquaintance with need or know-how about need. Nor need it be appreciation, understanding, and insight. Everyman's vocabulary distinguishes what the radical sociologist confounds. To think clearly about the relationship between knowing and doing requires that we remind ourselves of some common meanings.

Definitions:

1. *"To have information"* is to be able to state a fact.
   a. *Facts* are sentences of empirical predication for which, within some context, there is public warrant.
   b. Among the facts that count as information are *definitions*. Definitions become factual as they approximate those that say, "According to such-and-such dictionary, *this* word means *that*." This means that definitions are "more informative" as they become conventional. Implicit and private definitions do not count as "facts"; they may communicate, but they do not predicate.
   c. A class of facts included within the definition of "being informed" is the ability to identify (that is, communicate) the cues to which one is differentially responding.
      i. The distinction between "information" and any "communication" is required by the fact that organisms make differential discriminations without necessarily being able to verbalize the cues that guide their perceptions.[8] In common lexicons, we do not call discriminatory response "informed" unless the response can be accompanied by a symbolic expression that specifies the cues guiding the differential action.
      ii. It is this definition of "information" that leads one to say that information can be catalogued and reliably communicated. By contrast, "non-informative" messages contain signals with unclear (unconventional) codes. Thus, it may be held that music and art "communicate" (feelings, attitudes, or insights) without claiming that they are "informative."
2. *"To have knowledge"* is to be able to state a non-tautological empirical rule.
   a. The idea of "rule" means that the statement refers to more than one case of . . .
   b. The degree of "rulefulness" is never known. It can only receive more or less warrant.
   c. Warrant is indicated for each man by the confidence with which he risks action upon the empirical rule.[9]
   d. Tautology is difficult to define.
      i. Its definition depends upon a specification of "independent systems." Some of these "independent systems" are themselves matters of definition (of convention). Others are built into our physiologies.[10]
      ii. Given the assumption of "independent systems" of observables, a tautological rule is one whose predicate *(explanans)* is included within its subject *(explanandum)*.
3. *"To be acquainted with"* betokens familiarity. Its sign is recognition.
4. *"To know how"* is a phrase applied to any event or concert of events that is deemed probable of a person or thing in a delineated circumstance.
   a. "Know-how," in tongues other than English, may include the behavior of inanimate as well as animate objects.[11]
   b. When applied to persons, "know-how" refers to a range of actions from those that are largely combinations of reflexes to those that involve the conscious exercise of techniques. The phrase is usually reserved for application against some standard of competence.
      i. Examples at the reflexive end of the range include walking, swimming, or playing tennis.
      ii. Examples on the more consciously adaptive end of the spectrum include influencing people, building a house, and constructing programs of social change.
   c. The range of reference of "know-how" and its lack of a separate lexical indicator in Germanic and Romance languages enlarge the risk that Westerners will confuse habit, skill, and some

styles of competence with knowledge. This point will bear repetition.

5. A "*program*" is a regimen of assorted actions adopted as an instrument of purpose.
   a. This means that such a routine is called a "program" only when it has been planned, that is, only when it has been "thought out" (verbalized) by one or more persons *prior* to its being acted upon.
   b. Being "*planned*" means, in addition, that the recommended routine is designed to achieve an objective or objectives. This qualification excludes from our definition many large-scale enterprises engaged in the name of vague ends. Honorific rubrics like "equal opportunity," "mental health," and "human flourishing"[3] are examples of titles of group objectives whose ambiguity accommodates diverse activity and puts means beyond empirical test.
   c. A "program" may be acted out by one or more individuals. These need not be the same persons as the inventors of the program.
   d. This definition excludes those regularities of behavior that have histories but not purposes, that are, in short, crescive rather than enacted. Thus, the sociologist's "institutions" are excluded by definition because, while they may receive "purposes" as their justification, they are not "purposive" in the sense of having been *designed* to achieve some end.
   e. This description of a "program" is advanced toward the major objective of definition, namely, to set boundaries upon a word so that it may be more profitably employed. Such a definition in no way urges that patterns of routine behavior observed as systems must be either programmatic or not. In real life, as opposed to definitions of it, those systems of behavior called educational, industrial, judicial, and familial exhibit varying combinations of instrumental, reflexive, and expressive behaviors. This is to say that they are only in some part rational.
6. A "*policy*" is a set of principles for the guidance of actions, including the act of deciding how to act.
   a. The principles that constitute a "policy" state preferences. When verbalized, the preferences may be ethical, aesthetic, expedient, or some combination of these and other categories of ends.
   b. Policies as stated include varying combinations of ends-in-themselves (actions that require no further justification) and instrumentalities (recommendations for action in order to achieve some objective).

Proposals:

Distinctions have been drawn between the meanings of being acquainted, informed, knowledgeable, and competent, and between a program and a policy. With these definitions marking off frequently confounded types of conduct, it is possible to examine the relationships among them, if only heuristically, so that we may form a sounder judgment of the likely consequences of gathering information and inventing knowledge for social policies and programs. The probable relationships among these activities is presented as a series of propositions with their qualifications.

1. *Acquaintance may be positively associated with information.*

It is assumed that acquaintance requires, and acquires, information. However, such a working assumption does not deny that one may acquire information about another person or about things without acquaintance. One can gather data about Mt. Everest without recognizing the peak when he sees it.

2. *Information and knowledge are brought together by definition.*

In present terms, knowledge is built with information. However, information by itself is never enough to produce knowledge. The facts that count as information are not always rules. Until regularities are discerned among the facts, until a theory is imposed upon them, information remains something less than knowledge. It is possible, then, to be informed without being knowledgeable.

Policy makers are cautioned that "doing research" where this means "gathering data," is not the same as obtaining knowledge. The confusion is readily made. It is made out of desire, mixed with an inflated respect for the expert, and the easy leap from correlation to causation."[4]

3. *Acquaintance and knowledge have an unknown form and degree of relationship.*

There are styles of acquaintance and their association with knowledge is open. It can be cogently argued, for example, that some qualities of acquaintance befriend knowledge while others offend it. Thus, love and loyalty are qualities of acquaintance whose relationship to knowledge remains moot. While some students see love as

producing (or meaning) self-disclosure (Jourard, 1964), others have called love "blind." (Chaucer; Shakespeare). It seems fair to conclude that, insofar as the quality of personal acquaintance is expressive rather than purposive, neither information nor knowledge is of moment.

Comment on loving acquaintance may seem distant from our interest in the pertinence of knowledge to social policies and programs. However, let it be noted, for later amplification, that one reason, among many, why some knowing is irrelevant to some doing is precisely because love, under such varied titles as care, concern, or commitment, enters the political arena. Loving, to repeat, does not need knowledge, may be alienated by it, and hostile to it. This is yet another way of calling attention to the hostility of some values to some facts.

The quarrel about the limits of acquaintance as a condition of knowledge is more forcefully engaged where the thoughtways called "science" meet those of "folk wisdom." The contribution of "sciencing" to knowledge has been to extend the horizons of what can be known beyond that with which we are familiar. The natural history of physical science is a demonstration of improvement over folk knowledge, where improvement is gauged by added foresight and greater control over a wider range of events. Such success has encouraged the optimism that a science of social behavior would do likewise. In the present context, it can be stated dogmatically that no such social science has yet evolved. None, that is, that has lived up to the great expectations carried by the noble title.[15]

4. *Information and know-how vary in their association with the type of activity.*

On the reflexive end of the range of action, the relationship between know-how and information is slight.[16]

It is *assumed* that the relationship between information and know-how increases as one proceeds toward the purposive end of the spectrum of action. Even here, however, information is not enough to guide action effectively and the relationship between information and know-how is never unitary.

The model of the application of information to competence is a guidance system. In such a system, there is a program designed to achieve an empirical end. Information is used in such programs to test for deviations from course, with instructions, in such cases, to bring the activities back on target. *If* large-scale political movements were designed with clear objectives, then information might contribute to know-how. It is, however, a fact of life that mass collective efforts suffer mixed objectives and unclear ones so that it becomes difficult to decide what information is required to bring such quasi-programs to target. (See footnote 25).

5. *The relationship between knowledge and competence is far from clear and certainly less strong than such popular slogans as "Knowledge Is Power" assume.*

The association of knowledge with know-how is best analyzed in the separate spheres of the control of inanimate nature, interpersonal behavior, and collective conduct.

It has been argued that knowledge has demonstrably increased know-how when applied to inanimate things and that this success has emboldened men to think analogously of the possible powers of knowledge of the personal and the social. However, in the realm of interpersonal action, knowledge, and competence are only slightly related, if at all. "Getting along," being effective, being good and doing it — all signs of interpersonal ability — are not strikingly dependent upon knowledge. The illustrations of this failed association are many. A prime example is the psychological savant and the affectional idiot resident in one body.

The record of the social studies as applied to the behavior of aggregates of people challenges their borrowing the title, "science." What little knowledge men have gleaned that improves with common sense has not greatly increased our competence in the management of our collective affairs. Economics and psychology have advantages here over their sister subjects. This does not make them rigorous disciplines in the sense of studies that yield a high predictive accuracy. Rather, their advantage in forecast competence is by contrast with the meager prophetic utilities of anthropology, political science, and sociology.

The sign of social science impotence, and a source of it, is the difficulty of phrasing non-tautological empirical regularities of social behavior that have much greater generality or validity than the rules to be found in any civilized literature. One hastens to add that there are *some* rules discerned through the social studies, but they are few and, against the questions asked of their scholars, impertinent.

This first test of the utility of social science underwrites the second sign of its weak relevance for the public decision-maker—the failure of prophecy. It is here that the distinction between knowledge and information is most clear, for one hesitates to call

facts "knowledge" unless their knowing increases the accuracy of contingent forecast. The record, again, does not lend confidence in the expertise of social scientists – with some guarded qualifications.[17]

If the predictive scores of social scientists are no higher than those of laymen of like intelligence, the mantle of "scientist" is falsely assumed. Exposure of the masquerade has led some outsiders to cry "ideologist" and stronger names, and some insiders to run from reality toward religion.[18] Probably only a minority of students of the social scene continue efforts to invent knowledge out of information and to test the applications of their inventions without conversion to the faith that every social question can be answered or that any particular difficulty in living will be mollified with facts.

This descripton of the low knowledge-power of social science is not totally unflattering. We are reminded that the social science enterprises have enlarged our fund of information. They have probably influenced our ideologies (our "images of the world," if you prefer), which means that social science may have contributed to changes in our morals. These studies have also contributed to knowledge: they have allowed us to make *some* generalizations about the behavior of social systems not to be found in folk wisdom.[19] However, the impact of social science upon information, upon belief, and upon values may be quite separate from its knowledge-yield. We can, and do, believe different things without knowing more, or better. We can be more fully informed without being wiser.

With awareness of the limitations of the sciences of social behavior, one can look for the sources of this deficiency so that our expectations of the social studies may be brought into better alignment with their possibilities.

There are at least three categories of fact that explain the modest contribution of the social sciences to knowledge: the large inventory of folk wisdom, the kind of questions asked by social scientists of their studies which, in turn, determines where one looks for the answers.

There is a folk knowledge derived through familiarity with animate and inanimate worlds that cannot be assumed to be "all wrong." Without attempting to assess this common knowledge in all departments, it is sufficient to say that there is enough common sense around to make improvement upon it through a social science a hardy assignment.

The debility of social science is also ensured by the kind of question frequently asked about human behavior. The questions tend to carry a heavy load of evaluation, of approval and disapproval of the subject matter, a burden not so heavily put upon our observation of non-human events. To put it bluntly, the moral questions commonly asked of the students of man cannot be answered with the scientist's tools. Values cannot be *deduced* from facts, although information may influence values. There may be a *causal* connection between facts and values without there being a *logical* entailment between them.[20]

The demand that a social science answer questions of moral import imposes a constraint upon the search for answers. The moral motive restricts *where* one may look for the answer as well as the *kind of answer* to be permitted. This is part of what was meant earlier in speaking of the hostility of love and loyalty to information and knowledge. Some consumers of social science and the majority of its practitioners want knowledge to *change* what has been observed.[21] Knowledge, however, rests on continuities. *The demands made of social knowledge ask it to contribute to prescribed discontinuities with a guaranty of result.* A consequence of this desire is that an undetermined portion of what is known about man is disapproved. The strong expressions of disapproval have revealed the social sciences as subject to ideological contamination. An evidence is the development of taboo topics.

This constraint upon the kind of answer sought is also exemplified by the congenial assumption that major events have few causes. Our values that would intervene with social programs and change lives (principally the lives of others) evoke enthusiasm from the description of a small number of conditions constituting the source of what ails us. Multiplying the possible causes of our concerns not only makes the task of thinking about them difficult; it also frustrates hope. Such a model of social behavior is not politic.

This may explain why the social sciences (excluding multivariate economics and psychology) share with common sense the search for the causes of human action in "sparse systems." A "sparse system" is one in which the generators of change are few and independent of each other. It is more likely, however, that social systems are "dense," that the sources of action are *many, interacting,* and *non-uniform* in their interaction.[22]

A moral concern that urges action will be impatient with dense systems. Similarly, legislators who ask their social science advisors "what to do" will not be happily counselled by being told that the roots of the miseries they would correct lie entangled

in a multiplicity of disorderly interacting variables.

These constraints upon the advice to be gained from gathering data and seeking knowledge about social events direct us to the possible best uses of the social sciences as guides to policy and program.

Social science seeks to improve upon folk wisdom by drawing cognitive maps whose schema derive more from information than from acquaintance. The utility of the scientific map rests on the accuracy of its information, on the applicability of the information to the purposes of the map-reader (not all information "counts"), and on the manner in which the bits of information are put together.

Whatever improvement a social science has made upon folk knowledge has been due to its construction of more self-conscious, "clean" taxonomies, and the more reliable counting and more valid correlating of behaviors. *These actuarial skills constitute the expertise of professional students of social behavior.* Such a description of what goes with what, under specified conditions, is the most reliable value to be derived from politicians consorting with social scientists. The particular utility of these professional skills lies in the assessment of programs *during* and *after* their running. Within the realm of political action, such hindsight can contribute to foresight, particularly in the sense of telling us what will *not* work.[23]

Implications for Programs and Policies:

Programs are to policies as means are to ends. Where the ends are unclear,[24] any program can be justified and no program can be evaluated. Information and knowledge are irrelevant.

Ends become unclear as they are formulated out of plural and conflicting preferences.[25] This is a normal situation in a democratic environment. *To the extent to which public policies are thus ambiguously derived out of conflict, programs cannot be rationally designed.* Programs so generated become compromises, efforts to contain conflict. They are thus *intelligent*, but less than *rational*. Government activities taken in the name of such policies will be either temporizing — doing something in the hope that interests will be appeased and difficulties disappear — or ignorant.

Temporizing is not favored in the public Can-American philosophy, although "muddling through" and the practice of "masterly inertia" have British and Spanish credentials, respectively. (Moynihan's recent plea for an attitude of "benign apathy" toward American race relations indicates, however, that some North Americans can learn to think like their European cousins.)

Given the apparent prejudice against temporizing, we are assured that some proportion of governmental activity will be guided by ignorance. The assurance is supported by the fact that being ignorant has seldom prevented people from acting, and that, indeed, some ignorance seems positively to have encouraged action where knowledge might have prescribed indifference or patience. It is no surprise, then, to hear governments repeatedly charged with acting ignorantly and with producing the opposite of what they say they intend. Each man can inventory his own examples; a footnote will bear a few.[26]

The theme deserves repetition: information and knowledge can only be used if one "knows" what he wants. Ambiguous goals and divided goals cannot employ information as a guide to *rational* action.[27] In such a climate, the normal environment of ethico-political conflict, information will be used selectively and persuasively as propaganda, and the student of the social who would apply himself practically will come under pressure to take sides, to prove the points of those who purchase his services.[28]

These considerations underlie an assessment of how governments can best employ social scientists.[29] With allowance for overlap, these uses can be characterized as descriptive, exploratory, evaluative, and consultative.

(1) *The Social Studies as Descriptive.*

Social scientists may be employed as cartographers of the social scene to describe in greater scope, as opposed to the finer detail that is the artist's talent, how things are.

On a large scale, and with a broad brush, the professional student of social behavior can portray who is where, doing what, and, within limits, with what result.

As part of this expertise, the descriptive scientist can show governments how to improve the records from which these portraits are drawn. (The Matthews project, #5.1 in the SEOS research log, 1971, is an example.)

Description itself has diverse uses and unforeseen consequences. The advantage of such description is a function of the degree of consensus among the values of the description's consumers. High agreement about what is wanted can advantageously employ large, public descriptions of how things are.

The disadvantages of such description to an incumbent power follow. Description, fed as

information to a divided citizenry, is likely to exacerbate the divisions. Information can create difficulties as well as resolve them.[30]

(2) *The Social Studies as Exploratory.*

Exploratory (indiscriminate?) research is a privilege of rich governments. Only the affluent can afford to subsidize what have been called in sporting metaphors "scholarly fishing expeditions" and "shotgun research." The signs of such employment of the social studies are varied but related. They include the proliferation of research addressed to vague questions; the multiplication of inquiries about clear, but trivial, questions; and the subsidizing of research in which the questions and the answers may be clear, but where no policy is at hand to respond to the findings. Since no one with the power to act knows why the questions were asked, or necessarily agrees with the reasons for their having been asked, there is no one to act upon the answers.

This is the fate of much social research. It is an exercise. Such exertion probably characterizes the bulk of the investigation now being underwritten by the Human Resources Research Council. The objectives are lofty, hence vague, and probably at some distance from any question any minister has in mind or about which he needs to be informed.

The expectation of a payoff in action is low.

(3) *The Social Studies as Evaluative.*

Where public policy is clear and programs have been invented to fulfil the policy, the social scientist comes into his best use. His expertise, often mistaken for good judgment, is that of society's accountant. Where the counts are reliable, the accountant may be graduated to the role of societal actuary, calculating the odds on the results of alternative courses of action.

The advantage to government of societal accountancy lies in the possibility of eliminating inefficient programs.

A disadvantage to government lies in the interests vested in any program by those whom it supports. Inefficiency is sturdy and, in a democracy, powerful in self-defence.

Governments that would evaluate their programs should be advised that the evidence to date from "action programs" is predominantly negative. Most experimental programs that submit to evaluation are shown *not* to be achieving their objectives.[31] Here again ignorance is protective.

(4) *The Social Studies as Consultative.*

Governments have long employed counsellors — seers, jesters, and wise men.

A role of such advisors is to assist those who rule in the definition of difficulties. When one is troubled to decide, receiving a definition of the troubles is one step toward easier decision.

A second role is that of repository of information and knowledge. An informed counsellor is employed in the hope that information betokens knowledge, and knowledge know-how. It has earlier been remarked that this chain of assumption is weak. Nevertheless, governments will continue to seek advice, with good reason.[32] The advice is always tentative, of course, and the responsibility of governmental decision difficult to dilute in a climate of adversary politics.

A third advisory role is that of inventor of responses. The consultant becomes one of a consortium of "idea men," "brain-stormers" who may have new ideas for governmental responses to changing circumstances.

A fourth role of the professional counsellor is that of skilled investigator. Having assisted his client in the definition of his difficulties, questions of fact may flow from these definitions. The client may not know what is the case, although he now knows what is the question. Here the counsellor reverts to the functions of societal accountant and evaluator.

The difference between the employment of social scientists as consultants and social scientists as explorers is a difference in direction and hence, efficiency. The consultant and his client have agreed on the question before they search for the answer; the exploratory investigator does not know whether his client has a question or what it is. In the first relationship, it can be decided whether research will answer the question. In the second relationship, since questions are unclear, it is difficult to know whether, and what, data will resolve them.

It would seem more economical and more effective for governments to ask clear questions of their consultants than to subsidize promiscuous data-gathering.

Footnotes

[30] These ideas are expressed most recently and forcefully by Alvin W. Gouldner in *The Coming Crisis of Western Sociology* (New York: Basic Books, 1970). For example:

> A Reflective Sociology is and would need to be a radical sociology. Radical, because it would recognize that knowledge of the world cannot be advanced apart from the sociologist's knowledge

of himself and his position in the social world, or apart from his efforts to change these. Radical, because it seeks to transform as well as to know the alien world outside the sociologist as well as the alien world inside of him. (P. 489)

The social world, therefore, is to be known not simply by 'discovery' of some external fact, not only by looking outward, but also by opening oneself inward. Awareness of the *self* is seen as an indispensable avenue to awareness of the social world. (P. 493, emphasis his)

From the standpoint of a Reflexive Sociology, however, all research is 'contaminated,' for all are conducted from the standpoint of limited perspectives and all entail relationships that may influence both parties to it. . . . The aim of the Reflective Sociologist, then, is not to remove his influence on others but to know it, which requires that he must become aware of himself as both knower and as agent of change. He cannot know others unless he also knows his intentions toward and his effects upon them; he cannot know others without knowing himself, his place in the world, and the forces—in society and in himself—to which he is subjected. (P. 497)

A Reflexive Sociology is distinguished by its refusal to segregate the intimate or personal from the public and collective, or the everyday life from the occasional 'political' act. . . . A Reflexive Sociology is not a bundle of technical skills; it is a conception of how to live and a total praxis. (P. 504)

[2]The utility does not deny reality. The use of "models" and other constructions of the world is a way of opening the mind to possibilities not commonly envisaged. Models, built with assumptions and a selection of facts, have a usefulness, other than that of intellectual sport, when brought down to earth and tested for their contribution to the more satisfying manipulation of a real world.

[3]An illustration, and a demonstration of the logical indigestion produced by perspectivism, is Myrdal's philosophy of science. Myrdal has argued for some 30 years that values determine perceptions. (1944, Appendix 2; 1969; 1970) "*Value premises,*" he writes (1970:25), "*are needed even in the theoretical stage of establishing knowledge about facts and factual relations.* Answers can only be given when questions have been asked. A view is impossible except from a viewpoint. 'Things look different, depending upon where you stand'." (Emphasis his)

At the same time Myrdal believes in something called "reality," which, if we understand the common meaning of the term, refers to that which wishes will not alter. Thus he holds that (1970:15): "*The true institutional approach must work with concepts that are adequate to reality* from the beginning . . ." (emphasis his), and that (1970:27): "*The underdeveloped countries should be studied in terms that are adequate to the reality there . . .*" He concludes with this tangle (1970:27): "*The value premises determine only the point of view from which reality is studied.* That point of view does not determine, however, what facts and factual relationships are brought within the scope of observation and analysis. It is my conviction that economic study must be comprehensive enough to be adequate to reality, and that this reality is very different in the underdeveloped countries from what it is in developed countries. No value premises prescribe that a study should not be realistic and relevant to the maximum extent." (Emphasis his)

The source of Myrdal's confusions is his confounding of two propositions:

(1) Values determine the questions one asks which, in turn, influence the kinds of answers one seeks. This is an unexceptional proposal. It is hardly news and it would be difficult to find a scholar who has claimed the opposite.

(2) Values determine what is a fact, that is, what is the truth. If this were so, there would be no point in trying "to find things out" and "doing research" would be writing propaganda.

Myrdal and his fellow perspectivists believe that they have proved the second proposition ("There is no objective social research") by arguing for the first. With all the rest of us, they cannot maintain their position consistently. For example, Myrdal himself talks like a realist when he says (1969:93): "The relative importance of nature and nurture is a question of facts, and beliefs can be proved to be true or false by research."

[4]There are other modes of utterance, of course, that express more and indicate less.

[5]The independence of reality means at least three things, according to Lewis (1929: 193-194):

> . . . first, the givenness of what is given; our realization that we do not create this content of experience and cannot, by the activity of thinking, alter it. Second, it means the truth of those 'if-then' propositions in which the process of possible experience, starting from the given, could be expressed. The 'if' here depends upon our own active nature for its meaning, as has been pointed out, but the content of the 'then' clause, and the

truth of the proposition as a whole, are things with respect to which the knowing mind is not dictator but dictated to. I may confront the given with different attitudes and purposes; I may be differently active toward it and, starting from it, I may proceed into the future in different ways. But *what I should then find;* what eventuations of experience are genuinely possible; that is something independent of any purpose or attitude of mine. These, I seek correctly to anticipate in my present interpretation of the given. If they do not obtain in reality, my present 'knowledge' is false. Whether they obtain or not, is determined independently of my mind. If not, then it is not determined at all, and knowledge and error are, both of them, purely subjective and meaningless.

Third, the independence of reality means the transcendence by reality of our present knowledge of it; it means that I can ask significant questions about my object which *have* an answer when that answer is something which I cannot give. In terms of experience this means that, starting from the given in certain ways, I can safely predict the accrual of *something* the particular nature of which I cannot now determine. For example, if I examine the contents of this drawer, either I shall find a piece of chalk or I shall find none. So much I know; but I do not know now—and cannot discover merely by taking thought—*which* of these alternatives I should find true. There is that in the object which I do not know; I know something to be determined in reality which is neither implicitly nor explicitly determined in my knowledge of it. This, and all similar questions I could ask and could not now answer, witness the independence of my object. (Emphasis in the original)

[6]The quarrel carries on and will not end here. It is notable, for example, that Marxists continue to debate whether perception reflects reality or constructs it. Feuer (1970) writes of this as the struggle of the revisionist Marxists with Lenin's ghost and cites Lenin as having said, "Any healthy person who is not an inmate of an insane asylum, or in the school of idealist philosophers [holds to] naive realism, [a belief which] consists in this, that he believes reality, the environment and the things in it, to exist independently of his perception." (P. 25)

The issue is not confined to the Marxian fellowship wrestling with its sociology of knowledge. It spills over onto other social scientists who, recognizing that we work with models of our experience, argue as if all perceptions of the world were of like status, equally imaginary. Thus Boulding (1956:165) can be read this way when he writes that "The difficulty with any correspondence theory of truth . . . is that images can only be compared with images."

To repeat, no one acts upon this belief. We are saved from the endless comparison of illusions by the fact that some perceptual organization of the world is built into the organism. (See footnote 12). Every statement about how things are cannot be disputed as "just the other fellow's interpretation." Students of hermeneutics, of the interpretive act, have addressed themselves to this problem without losing hold of reality. For example, Hirsch (1967:37) tells us:

> Meaning is an affair of consciousness, and the fundamental characteristic of consciousness, as Hume, for all his psychologism acutely observed, is that it is always consciousness of something. One of the most brilliant passages in Coleridge's *Biographia Literaria* is his use of this insight to attack the empirico-psychologist notion of perception, according to which the thing that one sees when one looks at a table is one's perception of a table. How odd, observed Coleridge, when we always supposed that we were seeing a table! To speak of perceptions instead of "tables" is precisely the sort of misplaced sophistication that is found in the psychologistic account of meaning, according to which what one understands is really one's perception of or response to a meaning. But the remarkable fact of consciousness is that the objects of its awareness are not the same as the subjective "perceptions," "processes," of "acts" which are directed toward those objects.

[7]The indefinite modifier is required because no one knows how much intention generates knowledge. Knowing is "often" its own activity, satisfying curiosity as well as decision.

[8]This may not be true. It may not always be true of an individual's action upon his purposes. It probably is always the case when groups of people act in concert or in conflict.

[9]The literature is critically summarized by Eriksen (1960).

[10]A person regards a statement as "certain," says Frankfurt (1962:319), if he ". . . is willing to take the risks associated with the statement if there is nothing to be gained by refusing to do so."

[11]Evidence of this physiological "givenness" is to be found in Bouissou (1942), Brain (1950), and Pitts and McCulloch (1947).

[12]Hockett remarks (1968:62): "Chinese is more helpful than English for clearing this up. If a person knows how to speak Swahili, he *hwèi* speak Swahili;

if he smokes, he *hwèi* smoke; even of inanimate objects, one can say that a high wind *hwèi* (here perhaps 'is apt to') blow down a tent, or that an electron *hwèi* behave in accordance with the equations of wave mechanics."

[13]Anscombe (1958) advances "human flourishing" as the ultimate standard of a moral philosophy. She admits, however, that the idea defies definition.

[14]A prime recent illustration of this confusion is the popular interpretation given one of the findings of the second largest (in money and number of persons studied) social science project in history, the study of *Equal Educational Opportunity* directed by Coleman (1966).

The study reports modest correlations between a few measures of pupil achievement by ethnic group and the proportion of pupils in the school who are white. The low correlations 0.20 was used as the cut-off determinant of "significant association") have been interpreted by journalists and politicians, with some help from the writers of summary interpretations of the Coleman study, as demonstrating the favorable impact of desegregation upon minority-pupil performance. Without control for differences in initial ability, the reported correlations may well be spurious. (Nichols, 1966:1313). However, beyond that, civil rights groups and the "better" periodicals leaped to the inference that the enforced congregation of majority and minority groups *would produce* measurable improvement in the performance of ethnic minorities. Dyer (1968:53) comments: "This suggestion that the student-body correlates can be useful in generating hopeful hypotheses to be tested is not to be taken as in any way supportive of the flat assertion in the summary of the Coleman Report that 'if a minority pupil from a home without much educational strength is put with schoolmates with strong educational backgrounds, his achievement is likely to increase.' Quite the contrary. *There is nothing whatever in the Coleman analysis that can justify such an inference.* The Coleman study contains no data at all on the effects that might accrue from 'putting' minority pupils with different kinds of schoolmates. It is one thing to suppose that a pupil's attitudes and behavior reflect those of the peer group in which, because of innumerable circumstances, including possibly his own predilections, he happens to be; it is quite another thing to infer that if he is moved from one group to another, his attitudes and behavior will change in predictable ways. This is not to say that changing the mix of children in a school will not change the children in the mix; it is merely to call attention to the fact that the Coleman data, by their very nature, are incapable of providing any information at all on what changes will occur or the likelihood of their occurrence." (Emphasis supplied)

[15]This dogmatic statement will be accepted or rejected depending upon how "scientific" one expects his social science to be. Mazur (1968) calls sociology "the littlest science." Economics, when subjected to the test of raising predictive accuracy, also appears rather short-sighted. (Cf. footnote 17) Anthropology, history, and political science—despite their increasing urge to be "relevant"—have not been subjected to this measure of a science.

Nevertheless, practitioners of the social studies and their supporters in government continue to speak of their proved utility. Thus Dupree (1970), in testimony before the American Congressional Subcommittee on Science, Research, and Development, claims that "The social sciences must receive greater emphasis both because they have demonstrated increased effectiveness in the last quarter century and because the demand for the application has increased." (P.10) The word, "effective," is left undefined. The implication, carried by the term "science," is that the effectiveness is knowledgeable, an improvement upon political judgment. There are, of course, other meanings of "effective" in this context, such as "ideologically persuasive." The latter utility can be acknowledged without granting the former.

[16]For example, with respect to language acquisition, Lenneberg observes (1967:324):

> From these examples it is clear that the construction of proper sentences is not facilitated by telling a child how to do it. It must be admitted that no one knows how it *is* done . . . No grammar, old or new, furnishes us with a recipe of how to speak grammatically. There is no grammatical system available that could be used to help an essentially language-deficient person to put words together to form good sentences. So far, grammars merely specify the underlying structure of sentences and explain how sentences of different structure are related to each other. (Emphasis his)

[17]Sociology seems to have had its greatest success in prognosis when it counted observables and subjected the counts to the least theory—"least" in terms of untested assumptions added. In this light, it is illuminating to look back upon *Recent Social Trends* (President's Research Committee, 1933) and to review the extrapolations made there under the simple notions of an Ogburnian view of social change. With the exception of the population forecasts(!), the

findings have some prophetic accuracy. Whether the balance of predictive error against predictive improvement over common sense justified the cost of the project is another matter.

Psychologists have developed instruments that allow modest improvement over chance and folk knowledge in the forecast of scholarly and occupational success. Under the pressure of other objectives, these uses are now being called irrelevant if not immoral.

Economists have had limited success as short-range forecasters. Their expertise, given the time and dollar investment in it, has striking limitations. See, for example, Heilbroner (1966), Morgenstern (1963), Postan (1968), Schoeffler (1955), Zarnowitz (1967).

[18]This statement does not decry religion, a constant of human relations and a goal of even the radical social scientist who only rejects religion when it is so labelled. The comment remarks on an intellectual trend in which some history and more psychology and sociology are converted from inquiries as to how things were and are to persuasions as to how one ought to live. See, again, Gouldner cited in footnote 1. Shils (1967:90) comments: "For those who believe neither in Christianity nor in Marxism, sociology is becoming a mild surrogate religion. It is replacing prayer as a source of guidance, and those who need this consolation are not going to be finicky about the language in which it comes."

[19]For a brief inventory of such generalizations see Nettler (1970:197-198).

[20]There are three questions debated here:

(1) *The logical issue:* Do factual premises entail evaluative conclusions?

(2) *The evidential issue:* Do facts count as *evidence* for moral judgments? If so, under what circumstances?

(3) *The causal issue:* Does "having information" affect values?

The literature on the first two questions is extensive. Some of it has been treated by Nettler (1968:fns. 11 and 12). See also Beardsmore (1969). The most reasoned conclusion thus far seems to require a negative answer to the first question and an affirmative answer to the second with the contingencies unknown.

The answer to the third question is assumed to be affirmative, although the relationship has not been well tested. The causal connection is usually imputed by fiat as when Boulding (1967:886) tells us, "We cannot assume in any of the sciences that the development of more realistic and complex images of the world leaves the human value structure unchanged. Our image of value and our image of fact are symbiotic. They are part of a single knowledge structure and it is naïve in the extreme to suppose that they are independent."

It may be "naïve" to assume some independence of facts and values, but it may also be accurate. There seems to be no evidence as to the extent to which information changes values. As for the independence of the two, part of this inheres in their definition. (See Ladd, 1957:101-107) Independence also is warranted by the experience of two men agreeing on facts, but continuing to disagree on their value-implications. For example, in the very journal in which Boulding attributes a "symbiosis" to facts and values, there is an article by Kopkind (1967) addressed to the impact of the "future planners." In this, Kopkind can admit to some facts about the Negro family that Moynihan (1965) adduces. He admits these facts, but interprets them as gathered in response to a question that itself flows from certain values. The critic is left rejecting both the initial and the terminal judgments. In short, for Kopkind, neither the values that motivated the question nor the values recommended by its answer are acceptable, although the facts brought out in answer to the question are.

The degree and direction of any causal connection between facts and values remains an open issue.

[21]A bit of evidence may be seen in a study sponsored by the Carnegie Commission on Higher Education and reported in part by Lipset (1970). Professors were asked to characterize themselves politically as Left, Liberal, Middle of the Road, Moderately Conservative, or Strongly Conservative. 80.5 per cent of the sociologists responding called themselves Left or Liberal while 71.8 per cent of the political scientists did likewise. The respondent anthropologists and psychologists were in accord with Left-Liberal percentages of 69. + per cent, followed by the historians (68.7 per cent) and the economists (61.7 per cent). These figures contrast with that for the entire sample of responding academics among whom only 47 per cent so titled themselves.

[22]With modifications, this argument leans upon Simon (1968).

[23]Students will recognize this negative value of social science as deriving from Popper's ideas. Popper (1959: Section 15) reverses the usual way of thinking about scientific discovery as *affirming* something and argues for the contribution of scientific method

through "negated existential propositions" such as:

> "You cannot carry water in a sieve."
>
> "You cannot have a centrally planned society with a price system that fulfils the main functions of competitive prices."
>
> "You cannot make a revolution without causing a reaction."

In another place, Popper (1962:346) holds ". . . the main task of the theoretical social sciences . . . is to trace the unintended social repercussions of intentional human actions." This conforms with our description of the "best value" to be derived from the political consumption of social science. By "social science" here, one is referring to an information and knowledge yield. The ideological contribution of "social studies" is another matter.

[24] It would make an interesting study to count the number of official statements of government policy for which indicators of objective are mentioned. For example, Biderman (1966), in reading the report of the United States President's commission on national goals, 1960, could find mention of what might count as a sign of goal attainment in only 59 per cent of the explicit statements of objective.

[25] A prime, recent illustration is provided by one thoughtful advisor to governments, Moynihan (1970), who argues, wrongly in our view, that *policies* can be *rationally* evaluated more readily than *programs*.

The example Moynihan uses demonstrates how the *diverse* objectives of a policy are forgotten by the time one comes to the test of that policy's achievement. He writes:

> In a similar vein is the Family Assistance Plan which has, properly I would think, been termed the most important piece of domestic legislation to come before the Congress in four decades. The principle of the program is simple and it derives from policy.
>
> Families must have an adequate income, and this should be provided them with a minimum effect on their status as stable, self-determining units in society, and with maximum incentive to earn by their own efforts as much a proportion of their needed income as they possibly can. The President has accordingly proposed to place a floor under the income of every American family with children, to provide within that context a specific incentive to work, specific opportunities for work training and placement, and the absolutely crucial provision that this assistance will not be conditioned on dependency.* A family does not have to be down and out, busted and broken to get help, when often as not all that was needed at a particular moment was a relatively small bit of help at the margin.
>
> DEFINING SUCCESS
>
> Will FAP succeed? None of us could say right now. But there is one essential aspect to such a question —a question directed to a program that derives from a policy. Namely, that the question can be answered. Family assistance will have succeeded if, over the course of the 1970's, the steadily rising number of dependent families** in the nation begins to level off and then to turn down, both in terms of actual numbers and over-all proportions. It is as simple as that to define success for a policy. Yet such definition is the one thing that often defies all effort when applied to programs. (Pp.99-100)

On the contrary: Note that the pacifying objectives of (a) "minimum effect on their status as stable, self-determining units in society," and (b) "maximum incentive to earn by their own efforts"—these objectives are forgotten and nowhere mentioned in Moynihan's definition of success.

A semantic sleight of hand adds to the difficulty of evaluating a policy predicated upon such mixed motives. The words "dependency" and "dependent families" have been starred because their careless use camouflages and inflates the nature of the test to be applied to the policy proposed. In his first use of "dependency," Moynihan does *not* mean, as most of us do, "being dependent"; he means being "down and out." However, by the time Moynihan arrives at "dependent families," six sentences later, it is not clear what he means; yet this is his *test* of success!

Since the number of "down and out" American families is reported to have been *declining* (Brooks, 1966; Lampman, 1966), it is difficult to read Moynihan as meaning "busted and broken" when he writes "*steadily rising* number of dependent families." One is then encouraged to interpret Moynihan's phrase as meaning by "dependency" what dictionaries tell us: "A condition in which a person, not possessing the means of securing economic goods or services essential to the minimum standard of existence, is forced to seek and accept public or private assistance." (Fairchild, 1944)

Neither reading seems to make much sense in providing more than an intuitive measure of FAP's success.

The latter reading says, "If you take people off 'welfare' and give them a governmentally guaranteed income, they will no longer be 'dependent.'" Such

usage puts us where Orwell told us we were headed in *1984*.

The first reading, which we assume to be Moynihan's, reduces his proposed *test* of policy to a banality of great consequence: "If governments give people money, they may have more than they've got."

[26]*Doing without, or against, knowing: Some recent governmental examples:*

Moynihan on the U.S. "war" on poverty and delinquency (1969:170):

This is the essential fact: *The government did not know what it was doing.* It had a theory. Or, rather, a set of theories. Nothing more. The U.S. Government at this time was no more in possession of confident knowledge as to how to prevent delinquency, cure anomie, or overcome that midmorning sense of powerlessness, than was it the possessor of a dependable formula for motivating Vietnamese villagers to fight Communism. (Emphasis his)

Boulding on U.S. income redistribution (1966:47):

In the United States . . . we are only just beginning to realize that a great deal of our intervention in the relative price structure, which was motivated at least in part by a desire to redistribute income towards the poor, has in fact redistributed income towards the rich.

Friedman on fighting inflation (1971:72):

The Johnson Administration tried wage-price guidelines. The guidelines failed and were abandoned. The British tried a wage-price board. It failed and has just been abolished. The Canadians tried voluntary wage-price controls. Their Prices and Incomes Commission recently announced the program was unworkable and would be abandoned Jan. 1.

Yet in the U.S., the Committee for Economic Development, an influential organization of businessmen, issues a report recommending (though with some vigorous dissents) a wage-price board on British lines and voluntary wage-price controls on Canadian lines. Arthur F. Burns, chairman of the Federal Reserve Board, supports the recommendation for a wage-price board. The Council of Economic Advisers' second "inflation alert" uses stronger adjectives than the first to describe price developments. Mr. Nixon, in a speech to the National Association of Manufacturers, tries mild jawboning, calling on labor and business to exercise restraint in the national interest.

Surely, this must mean that the experience of the Johnson Administration, of Britain and of Canada is the exception, that there are other examples of the successful use of incomes policy to slow inflation. Not at all. I do not know a single successful example and the current proponents of an incomes policy do not claim that they do. The refrain is rather, "despite the limited success of these measures elsewhere, they offer promise."

SOFT PROMISE

If the promise is not based on direct experience, is it based on economic analysis? Hardly. Economic analysis largely reinforces experience. It suggests that voluntary wage and price controls are likely to be honored in the breach—because those who observe them will suffer at the expense of those who do not—and that compulsory wage and price controls simply repress rather than eliminate inflationary pressure.

Lundberg on "teaching" the Germans (1948:699):

We destroy productive machinery in Germany at the very time that relief plans to replace such machinery are under way.

Jencks on teaching students (1966:21):

While the authors (Coleman et al.) make no policy recommendations, their diagnosis of what makes students learn is at odds with almost everything legislators, school board members, and school administrators have believed in recent decades. Not only that, but it is at odds with what most contemporary curriculum reformers believe. If the report's analysis is correct, then most of the money now being spent to improve the public schools is going down the drain.

Moynihan, again, on fighting poverty (1970:99):

The federal government has had since 1965 a formal policy of abolishing poverty. The Office of Economic Opportunity was established with that purpose in mind. But until this year almost a third of the expenditures of the OEO were provided, in effect, by income taxes collected from the poor! Since it has been in the nature of the "services strategy" so much in evidence in early OEO programs to hire middle-class persons to be of assistance to poor persons, the actual *income transfer effect* of many of these OEO programs was to take money from farm laborers and give it to college undergraduates. No one intended it that way, but that was the unintended consequence of programs being operated in the absence of policy. (Emphasis his)

[17]The adjective, "rational" is emphasized. To be rational is to act purposively toward the achievement of an empirical goal with the most economic means. Some philosophers assess degrees of rationality by the "consciousness" (quality of information) underwriting the choice of means. That is, the accidental or lucky employment of efficient means is deemed by definition to be "less rational" than "knowing" from evidence which means are the most appropriate.

Since much human action is fueled with moral and aesthetic motives—motives that are ends in themselves rather than means toward empirical goals—*the justifications given for these actions need not be informed.*

It is true that justifications consist of "good reasons" and that these "good reasons" sometimes employ facts. But this employment is justificatory—"apologetic" in the Greek sense—and *the facts given as reasons for action do not inform the action.* It is as with some judicial action a matter of "decisions first, reasons afterward."

Students of the social sometimes study behavior as though all of it could be understood through a model of rationality. This application ignores, where it does not deny, that moral and aesthetic conduct may fall outside the sphere of rational behavior. *People do not act only to achieve something.* Some action is end-in-itself. Asked, after the act, to justify it, we give "reasons," some of which may be facts. But we should not be deluded into confusing the justification with the motivation.

Examples of the delusive temptation are apparent where morality meets the law. Here the rational question asked of law is, "Why are you doing that?" Meaning: "What ends do you intend to achieve by enacting that law?" However, insofar as law *expresses* morals, *this* is its "reason," and the application of an instrumental question to an expressive action evokes a false answer.

One sees this confusion in the debates about the laws concerning pornography and capital punishment. The debates take the form, "If the law is enforced, what is the result?" *This form of question obscures the fact that ethico-legal action expresses attitudes as well as it intends consequences.* When Hannah Arendt concludes her study, *Eichmann in Jerusalem* (1964), she justifies the trial and execution of the Nazi offender by saying, ". . . to the question most commonly asked about the Eichmann trial: What good does it do?, there is but one possible answer: It will do justice." (P.254)

[18]One hesitates to document this stereotyped scenario. Each illustration runs the risk of losing its point by being labelled "exceptional." Illustrations are also disqualified by being attributed to the machinations of "bad guys," those in the employ of the "military-industrial establishment," whereas, it is assumed, "good guys," the ones on our side, do see and tell things more accurately.

At the risk of appearing excessively critical of one illustrious counsellor to governments, the dangers of using data to "prove points" are illustrated again in Moynihan's article on "Policy vs. Program" (1970). Moynihan asks whether race relations in the United States are getting better or worse. He concludes, "the data, such as they are, argue that indeed things are *not* getting worse. To the contrary, the Newsletter of the University of Michigan Institute of Social Research recently summarized the findings to this effect by Angus Campbell and others of the University's Survey Research Center, which we would all, I think, acknowledge as one of the four or five leading institutions of its type in the world." (P.97, emphasis his)

The opinion surveys referred to report changes of a favorable nature in what respondents said between 1964 and 1968 about residential, occupational, and school segregation affecting them. Here again a poll of *what people say* is used as evidence of *what people do and how things are,* an intellectual device long discredited. (LaPiere, 1934) Certainly many observations of how "the races" are behaving toward each other in the United States challenge Moynihan's optimistic conclusion. A sample of these observations will be found in Carithers (1970), Cowgill (1956), Hill and Feeley (1968), Pettigrew (1969), or any newsmagazine. For example, *The U.S. News and World Report* (1970) carries a feature headed "As Violence Spreads in High Schools" and the *National Review* (1970) editorializes:

> A spot-check survey just completed by the Office of Education failed to find a single integrated high school—anywhere—that was free from racial conflict.
>
> A pattern of "resegregation" exists all across the nation. . .

[19]The phrase, "best employ," does not exhaust the uses of the social studies. It has already been remarked that the social sciences may be used as ideological weapons when the cautions of truth-seeking are sacrificed to the urgings of morals. It has also been noted that the values transmitted through the teaching of the social studies may have an impact upon images of society and hence upon social action. These latter consequences are largely unintended, difficult to evaluate, and as likely to be subversive as consoling.

The last possibility has not escaped some men of power. Thus Pope Paul has been quoted (Associated Press, 1971) as denouncing "sociologists whose surveys help set off turbulence and dissent in the Roman Catholic Church." These studies, he added "can generate a moral uncertainty, socially very dangerous."

[30]This may be a reason for the Marquis de Sade's having written somewhere that "It is a danger to love man and a crime to enlighten him."

[31]Weiss (1970) summarizes the fate of much evaluation research in the United States. She points to the ever-present dangers of the politicization of such research—that "academic evaluators . . . bend the purposes of the study to suit their own disciplinary interests," that there is "pressure to vindicate the program," (PP. 60-61), and that the investigator is encouraged to leap from the data to recommendations, a long jump encouraged as well as obstructed by preferences and for which feat the societal accountant is ill-trained.

Weiss also notes:

> Perhaps the most serious political problem of all is that evaluation results, with dismaying frequency, turn out to be negative. Over the past several years, careful and competent studies have shown few positive effects from such varied programs as psychotherapy, probation services, casework, school desegregation, public housing, and compensatory education. Eilinson (1967), reviewing the results of ten of the most competent and best known published evaluations, found that none of them demonstrated much success. To judge from evaluations, most action programs do not make much change in the behavior of individuals and groups. (P.62)

[32]The good reason for seeking advice includes the hope that it will be sound, but it also includes the comforts in times of trouble of talking things over with a sympathetic auditor and the possibility of dissolving responsibility for decision in a chain of agents.

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The Modes of Feeding Research into Policy

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FEBRUARY 1971

The Modes of Feeding Research into Policy

It is only fair to begin with a warning that this paper is mainly a piece of casual empiricism. It is based on a number of years of first-hand experience of two federal government agencies by which I was employed and on observation during the same period of several other, neighboring governmental entities. So far as the paper has an underlying structure of theoretical ideas, these ideas will be seen to have been drawn, not primarily from my own discipline of economics, but from such fields as political science, sociology, and behavioral psychology. The skeleton of the piece is thus largely a pastiche of borrowings by an amateur.

What excuse may be offered for such unprofessional conduct? The excuse is that the more I think back over my own experience in government, the more it seems to me that what chiefly goes wrong in the linkage between social science research and the making of public policy has relatively little to do with the substantive content of the economics and other social sciences that actually gets fed into the decision-making process. Not that the present state of economic knowledge is an ideal one from the standpoint of its usefulness to policy-makers; obviously, this is far from the case. We have a few things yet to learn about little problems such as reconciling full employment with a politically and otherwise tolerable degree of price inflation. But that is a genuine, substantive policy problem, shared by many countries. A more perplexing failure in Canada, it seems to me, is the failure to apply more effectively various pieces of the existing and well-established body of economic knowledge that are both available and relevant for policy.

For this failure, there appears to be no single, simple explanation, such as the machinations of base political expediency—or, if you prefer, the rude irruptions of democracy in the raw, advocating short-sighted and ultimately self-defeating action over the thoughtful protests of more far-seeing mandarins. This does indeed happen, and can do so sometimes in rather subtle and hidden ways, but as a total explanation it is inadequate. No, the blame, if blame there is, must be more widely diffused. Some of it, for example, must fall upon economic research people themselves, who are by no means immune from such typical intellectuals' vices as cliquishness, "holier-than-thou-ness," preference for unwarranted degrees of abstraction because they are safer and less messy, love of the status conferred by knowledge-monopolies, reluctance to come clean with the popularizer—that increasingly necessary figure of our day, who if he sometimes makes a deplorably vulgarized mish-mash of other people's good ideas, is also disliked for being the trust-buster who lets the public in on the secret and thus devalues the monopoly assets. In that market place for scholarly work where the chief currency is not cash but original publication, you get few marks for writing down what every man and his dog already know—unless perhaps you can do it with a degree of perverse abstruseness that constitutes an original work in itself.

In thus turning on my own team, I do not, of course, mean to exempt the politicians and the higher mandarins: the blame, as I suggested earlier, is widely shared. It is already time, however, to stop considering this important matter in terms of personal blame and guilt. Except in odd, isolated instances, conspiracy theories and analyses of the failings of particular individuals will be of little help to us here. What we have to explain is a degree of *system* failure, the system in question being essentially a very complex information system that heavily conditions (though it is also conditioned by) the behavior of the human actors involved in it. If we can improve the structural characteristics of the system, we should be able to improve both the related human behavior and total system-performance.

Since I believe that people often nurture over-simplified and idealized notions about how government uses (or could use) research results, and about how people with social science research skills working inside government spend most of their time, I shall try to construct a more realistic, though still very simplified, model of the system. Before I do, however, I should like to make two points that seem to me fundamental.

The first point has already been well articulated by Alain Enthoven (1963). While he was writing of the work of economists in the United States Department of Defense, and one could wish for a more congenial example, his statement has general validity in government:

> . . . the tools of analysis that we in Defence use are the simplest, most fundamental concepts of economic theory, combined with the simplest quantitative methods. The requirements for success in this line of work are a thorough understanding of and, if you like, belief in the relevance of such concepts as marginal products and costs in complex situations, combined with a good quantitative sense. The economic theory we are using is the theory most of us learned as sophomores. The reason Ph.D's are required is

> that many economists do not believe what they have learned until they have gone through graduate school and acquired a vested interest in marginal analysis. (P. 422)

In other words, a lot of the social science that government uses, or should use, is pretty simple stuff, which does not require a massive effort of empirical research to prepare, but which does require fairly highly educated people to believe in and put it across.

My second point has to do with the less simple stuff and can best be made in the form of an anecdote. About ten years ago, when the Federal Reserve Board in Washington was first getting into the construction of econometric models to simulate mathematically some aspects of the operation of the U.S. economy and its financial sector, I spent a fascinating hour with a research man at the Board who had been identified as one of the leaders in this new endeavor. He was clearly very knowledgeable about it, and towards the end of the hour, I unthinkingly used some such phrase as "econometricians like you." He reacted immediately, saying, "Good heavens, I don't consider myself an econometrician. The real econometricians are working way out there on the frontier. I'm just a bridge-man. I try to understand the essentials of what they're doing and then communicate it to the management and the other research people who understand it less than I do."

Today, a general appreciation of what econometricians do is more widespread in government than it was then. But we still have a critical need for "bridge-men"—for first-rate popularizers, if you like. The communication of ideas in reasonably undistorted form, from the frontiers of knowledge to where the policy action is, virtually defines what ought to be one of the principal strands of relationship between social science research and government.

The question of how one develops good bridge-men is thus important to our problem. To some extent, they can be recruited from the ranks of partially superannuated frontiersmen. But not every frontiersman has the right qualities, and if pushing back the frontier is still what he likes and does best, the system should motivate him to stay with it. To be an ideal bridge-man, you must understand a lot about the city as well as the frontier. You must be literate as well as numerate—at home with the numbers, certainly, and well-read in recent theoretical and applied economics, but also in touch with day-to-day policy problems and enough of a journalist to be able to communicate forcefully and effectively with people of action and with the mass media. Given the range and continuing rapid development of present-day economics, this is probably an impossible set of qualifications, and no single person should be expected to measure up to it fully.

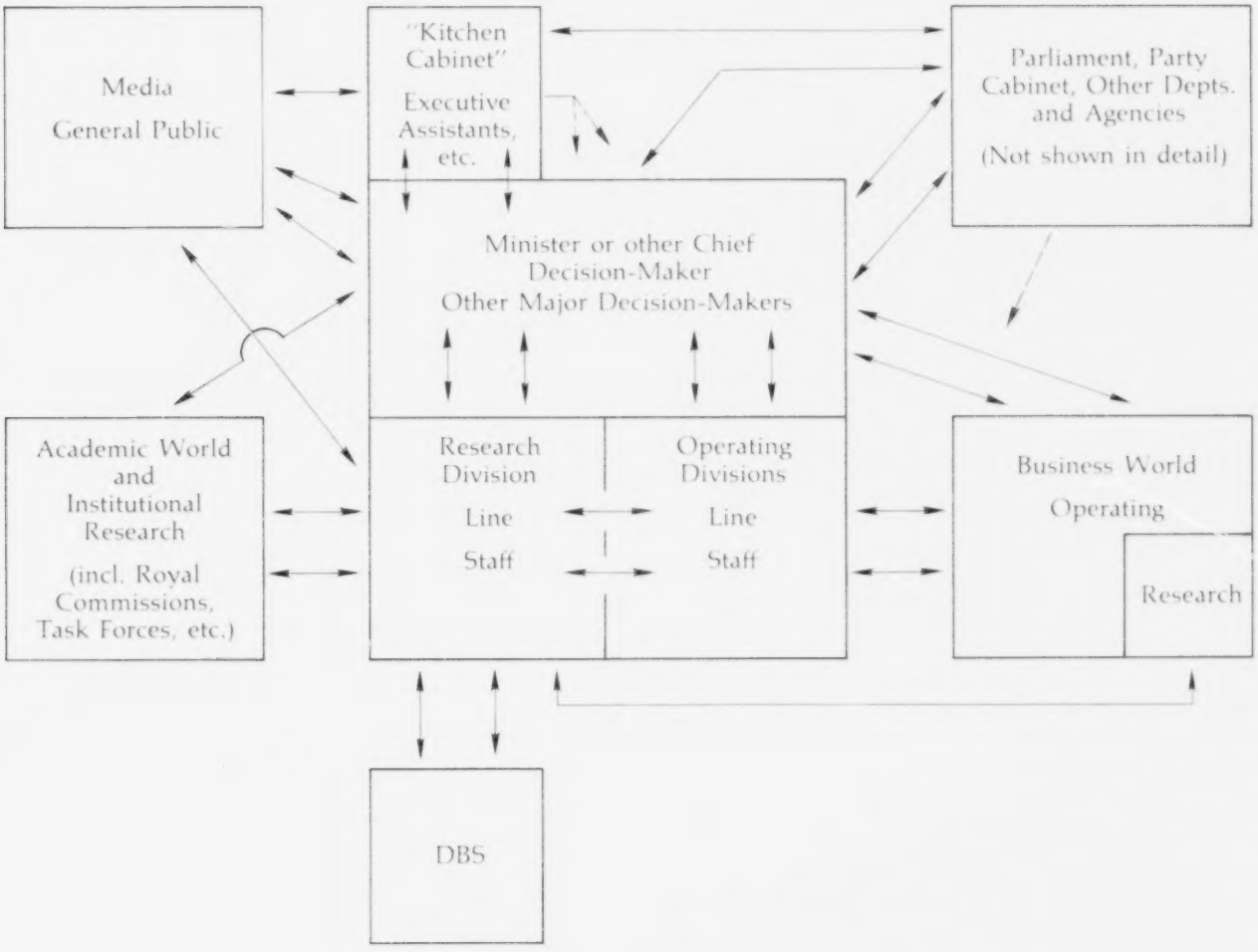
Sometimes, in Canada, one gets the impression that many frontiersmen simply assume that bridge-men will appear from somewhere and perform their necessary translative and communicative functions. "My work is ended—I have spoken truth—now let it prevail." In the United States, a country exceptionally well-endowed with high-class business journalism and with economic popularizers of learning and talent (one thinks of people like Walter Heller and Robert Heilbroner), this may be a legitimate assumption. But, in Canada, if you speak economic truth in a language comprehensible only to the elect, it may prevail very little. So far as economic policy is concerned, it may mostly just sit there. This is a country where we cannot safely assume that the bridging function and the conversion of ideas into practical policy applications will somehow be performed by somebody. It must be planned for and consciously built into the system.

Even so, it may not be out of order to suggest also that frontiersmen do more of their own bridging. Economics has always been a very applied science, and it is striking how consistently most of its major figures—Smith, Ricardo, Marshall, Schumpter and Keynes, for example—managed to encompass both a profound knowledge of theory and an intense concern with more or less contemporary policy issues. Their minds appear to have shuttled constantly between the theoretical and the applied, deriving from their contact with the latter some of the divine discontent that led them to question and subsequently to revolutionize the theory. In our own day, too, it is notable how much economists of world repute care about the application of economic ideas and how little, if at all, their academic reputations appear to have suffered by the propagandizing and pushing that they have sometimes engaged in on behalf of policies that they believed to be right. Not just the Galbraiths, but the Solows, the Tobins, and the Friedmans have had plenty of direct intellectual commerce with politicians and the press and have ground out some mean pamphlets (or their modern-day equivalents) on occasion. Applied economics is more often than not *political* economy that has to be fought for at

the level of practical politics and administration. If the idea is sound, there need be nothing inherently dirty about helping to fight it through into policy. The main valid reason for not lending a hand at this stage is a genuine lack of talent for the kind of work involved.

But let us push on to the model, as illustrated in Figure 1. It is, as suggested earlier, a drastically simplified model. For example, the larger, democratic political process of which it forms a part is merely sketched in and hinted at— " . . . far off, like a dull rumor of some other war." Also, the model is laid out in terms of a single department or agency, with little specific account taken of such institutions as Cabinet, Cabinet committees and Treasury Board. One could (and at some stage should) build an entire specialized model of the multi-level relationships *between* various federal departments and agencies, and between the counterpart departments and agencies of federal and provincial governments. Still another complex of relationships would run to and from the counterpart departments and agencies of foreign governments and of certain international organizations such as the Organisation for Economic and Cooperative Development and the International Monetary Fund.

SELECTED INFORMATION CIRCUITS OF A GOVERNMENT DEPARTMENT OR AGENCY



N.B. Not shown—relations with other levels of Government of Canada, foreign governments, and international organizations.

A particularly noteworthy omission, in the Trudeau era, is that of the lines of relationship to the Prime Minister's Office and the Privy Council Office. Canadian political scientists should be urgently concerned with studying the implications of the recent expansion and development of these two offices. It is also much to be desired that Mr. Marc Lalonde should some day write a book!

Returning to the diagram as given, this admittedly truncated picture may, nevertheless, serve some useful purposes. It is meant to illustrate four principal theses in particular:

(1) That the results of formal research in the social sciences are only one of a number of major informational streams flowing into the decision-making process, the relationship between the various streams being in some degree competitive;
(2) That people in government with training in social science research spend a lot of their time—some justifiably, some less so—doing things other than formal research;
(3) That research inputs nowadays get to the top decision-making levels through a variety of channels, sometimes moving up through the internal hierarchy via normal capillary action, and sometimes flowing in more directly from the outside through such intermediaries as Royal Commissions, task forces, party thinkers' conferences, and publications specializing in high-class popularization;
(4) That research management in government necessitates a number of difficult recurring choices, involving such things as the relative emphasis on research and non-research functions within the department or agency and the relative reliance to be placed on in-house and external research, respectively.

One further general point which does not readily emerge from the diagram but which must be made at the outset is that, except where time presses very hard, the development of policy is typically not a straight question-and-answer, challenge-and-response operation, but a much more iterative, drawn-out, and interactive process—where, for example, answers sometimes precede questions. It would be nice if policy problems invariably manifested themselves clearly through internal and external information media and through Parliament, if decision-makers converted the problems into well-formulated questions, and if research people then came up with firm answers capable of immediate policy application. Unfortunately, it rarely happens that way. The problem may not have been accurately identified in the first instance, the question may be vaguely formulated, and the research people may disagree with the question both because they think it is the wrong question and because in any case their data and techniques only permit them to answer properly some other question. But they may nevertheless make shift to cope, because if they do not, the decision may go ahead without them and if this happens too many times, they may no longer be asked to contribute.

As the diagram indicates, the results of virtually all in-house research and of much, though not all, external research relevant to the organization must move up through a hierarchy.[1] This is not to say that "top people" may not sometimes reach down into the research division, or even outside the organization altogether, for a direct input from some frontiersman; they may indeed, but members of the intervening hierarchy will usually exercise their right to be heard—to express reservations and qualifications, and otherwise to supply further inputs. This is, of course, especially likely to happen if some significant policy decision is involved. Thus, the hierarchical structure remains an important factor even if it sometimes pleases top people to rap with their young Turks and in this way possibly sow some mild consternation in the middle ranks.

It is not just a wiseacre platitude but a point of considerable operational importance that people change as they move up the hierarchy—rising, for example, from frontiersman to bridge-man to administrator, decision-maker and man of action. They grow older and more experienced, and usually more conservative, cautious, and sceptical. The changes in the nature of their responsibilities also help to produce the change of outlook. What once were the amusing, inexplicable hang-ups of a former senior management become, in some cases, real problems.

Regardless of whether one remains a civil servant or moves into the overtly political zone, the greater one's seniority, the more one usually has to develop the habit of thinking politically, and not merely in the sense of spending a great deal of time second-guessing Cabinet and Parliament. The *internal* politics of the organization—the reconciling of the conflicting claims and interests of its major personalities and sub-organizations—become more important. Part of the developing scepticism that comes with seniority stems from a growing appreciation of the vanities, biases, blind spots, and special interests of the individual divisions, including the research division. Even more important, however, at any rate at the

very senior levels, become the *external* politics: the survival and influence of the organization in the power structure of government as a whole, the fitting of its policies into a larger framework of government policy, the growing need to keep in touch with significant decision-makers and opinion-molders outside government.

None of this is to say that one's background—the particular set of "base steps by which he did ascend"—does not continue to exert some influence. Old loyalties persist to some degree, but the attitudes may be complicated. Ex-researchers, for example, may try to keep up with their fields and like to demonstrate periodically that they can still do it better than some of these young fellows. But it is in the end a losing and frustrating battle. Knowledge marches on; the old field becomes increasingly mathematical and otherwise impenetrable; the hungry generations tread one down. Meanwhile, the burdens of administration leave less and less time for keeping up, so that increasingly one must receive information from others, in fairly processed and even popularized form, on trust. It takes a well-adjusted personality to accept this with reasonable equanimity and not retaliate on the hungry generations by cutting oneself off from them, spending an excessive amount of time on various kinds of high-level politics, elevating one's power of intuitive judgment into a more universal guide than the record justifies, and pronouncing increasingly frequent anathemas on academics and other ivory-tower intellectuals.

After all allowance is made for archetypical managerial failings, however, it has to be said that the view from the top *is* genuinely different, and that research people, having different goals and alternative career opportunities, and viewing the organization from a different vantage point, are virtually bound to deliver a product that occasionally disappoints. Sometimes, for example, it can seem very negative and unconstructive, effectively exploding some myth or rule of thumb on which management has been wont to rely, but supplying no useful replacement.[2] But even if it is a more positive and helpful product, it is unlikely to remove more than an appreciable part of the uncertainty surrounding most government decision-making, and the manager will be quite right in not wishing to rely upon it exclusively. He will seek further information—will want to consider the views of his operating divisions who after all must implement the decision. He will consider, too, certain factors more visible, as he thinks, to him than to his research people—the broad political and Parliamentary picture, the external situation of the organization, the relationship to other government policies, the possible impact of this decision on the organization's influence and ability to make further impending decisions, and so forth. He may well want, also, some outside opinions—some fresh points of view. Here he may run a danger of being taken in by some very superior con-man from the business, party-political, or even the academic world, whose apparent knowledgeability and assurance contrast so favorably with the somewhat mixed track records and protective tergiversations of his internal advisers. There may never be occasion to discover that the visitor has also some bad forecasts to his discredit and that the large wealthholdings which help to confer on him such winning self-confidence and reputation for practical acumen have for the last generation increased no faster than the Toronto industrial average.

In studying the research-policy link, it is important to note that many of the information inputs that a management typically demands from its research division are not the results of formal research as an academic would understand research, and that many of them are not really inputs into current decision-making, at least at the policy level. Management seeks many things from its research division in addition to policy-oriented research: it seeks them there because the production and processing involved require writing, and computational and other skills that are usually more abundant in the research division than elsewhere. One major demand is for relatively simple current economic intelligence—for information on what has been happening to Gross National Product (GNP), industrial production, interest rates, unemployment, prices, wages, housing starts, and so forth. There can also be frequent demands for help in drafting legislation, reports and speeches, and in developing various descriptions, explanations, and justifications of past policy for use in Parliament, in appearances before Parliamentary committees and other investigatory bodies, and in media interviews. It can be important in many contexts for a management to "show knowledgeable"—to induce favorable answers to the recurring questions, "Does this guy know his stuff? Is his department with it?" Thus, while it may be hoped that there will quite often be some delivery of useful information to the public, the main purpose of many of these exercises is really the maintenance of the organization's prestige and influence. This is an unavoidable function, but if not carefully

controlled it can be terribly consumptive of the time of skilled researchers.

Let us now look a little at the functions of the director of the research division.[3] He is a bridge-man *par excellence*, striving not only to link his own division to the higher managerial levels, but also to connect informationally the organization and the larger social-science community outside. He has various difficult choices to face. Life would be much easier if he could concentrate simply on the carrying forward and dissemination of high-grade, long-term research, of a type likely to win the respect of the outside academic community and attract a steady inflow of first-rate candidates for employment. But, unless he wishes to be a mere prestige ornament without operational importance, he has another vital function: that of maximizing the conversion of research into policy and reinforcing the status and influence of his division within the organization. He must try to meet well the various specific informational demands of his management, including not only the routine provision of current intelligence, but also the more ceremonial and "show-knowledgeable" demands without allowing his division to be overwhelmed by them, to the detriment of longer-term research and the intellectual quality of his staff. He must try to convince the management to accept and demand what he considers to be a more significant and better balanced input of information from his division. Being in a competitive situation *vis-à-vis* the operating divisions and various outside sources of information feeding into management, he and his senior divisional associates must also devote time and attention to "showing knowledgeable"—to acquiring a sufficient understanding, not only of their own designated field of activity, but also of the organization as a whole and of its environment. They will then be much harder to put down as mere ivory-tower advisers. Fortunately, much of this nitty-gritty knowledge will also be of major substantive value for research purposes. But its acquisition, which generally has to be in considerable detail, can be very time-consuming.

The job, then, is not an easy one. There is much economizing of resources to be done. If he is a conscientious person, the research director will also feel himself to be a sort of inside representative of the larger research community—somebody whose job it is to put more research content into government policy and to do his best for such good researchers' goals as clarity, logic, honest thinking, respect for facts, and plentiful disclosure of information to the public. Having regard to the well-organized and articulate "producer" interests that press their views upon his management, he may also conceive it his duty to present a broader, more consumer-oriented picture of the public interest.

The larger research community can also help the research director, if they will. They can listen carefully to what he is able to tell them, study his problems, and those of his organization, closely, and, by making timely and persuasive noises in the right places, help to promote the adoption of better policies. To do this, they may well need some first-hand acquaintance with the organization, the purpose of which should not be seen by either party as that of brainwashing the potential critic and rendering him sympathetic to the prevailing official view, but rather as that of making him into an informed critic whose later considered opinions will not only be more valuable, but will also contain the kind of internal evidence of practical knowledge that is the best defence against charges of ivory-towerism.

Since management receives information inputs from many sources, both internal and external, an outside researcher anxious to promote a policy idea should try to do so through as many channels as possible, at various levels of popularization. The idea should indeed be inserted into the organization via the research division, but it should also be injected at other points such as party thinkers' conferences, and be given media exposure. As Commons and Senate committees grow in importance and secure permanent staff (both developments much to be desired), they too should become good input points. A certain amount of repetition does no harm. It helps to set up multi-channel, reverberatory effects and start fashions in opinion. Busy people are often intellectually fashion-conscious; they lack the time to be more value-minded consumers. The addition of a touch of style and trendiness to a worthy idea may help it to make competitive headway against the other kind.

Another important aspect of the external relations of the research division concerns the choice of what to do and what not to do "in-house." A basic core of relatively fundamental research is necessary for the balance and health of the division, ensuring that it will not deteriorate into a mere current intelligence and speech-writing unit, unattractive to first-rate young people. Obviously, however, there are limits to the amount of heavyweight work that can be done in the division. Such work should give some promise of being ultimately useful to the organization, and it should not duplicate to any great

extent research work being done outside. An important part of the division's task is indeed that of simply keeping up with the field and communicating relevant new knowledge as it is developed externally.

There will be cases, however, where a need is perceived for new research work to be initiated, but where it is not expedient for the work to be done in-house (perhaps on account of lack of suitable staff), and where, therefore, some kind of contracting out must be contemplated. Sometimes, in rare instances, the work can be performed satisfactorily on a "solo-practice" basis by a single academic working out of his university. There are, in fact, people who are sufficiently self-starting, undiscouraged by isolation, and blessed with an intuitive sense of what is needed that they can perform highly valuable work on this basis. Most researchers, however, need periodic guidance, expert criticism, help with statistics, and general encouragement; in any case, a growing number of economic research jobs these days, especially those involving much quantitative analysis with computers, must be tackled on a team basis.

Thus, contracting out often involves the assembly of a critical mass of skilled people, and careful arrangements for coordination and back-up. The chances are that somebody "in-house" is going to have to devote a lot of time to keeping the project moving forward satisfactorily.

This might be a suitable place to note some of the larger-scale methods of contracting out or "laying off" research work that are employed by government. On rare occasions, it is possible to assemble a critical mass of researchers out of the members of a single academic department at a single university. One of the team may act as local manager and coordinator. This worked very well in one case with which I am acquainted, but for obvious reasons the opportunity to use this method does not present itself very often.

A research-based *task force* can be deployed quickly and flexibly to deal with some relatively urgent problem. A disadvantage is that, operating as it usually does inside government, with much of its work confidential and perhaps also done in some haste, the task force may not command the same public respect as, say, a Royal Commission. Whether justifiably or not, it is likely to be more open to charges that it did not proceed thoroughly and impartially, but instead sought to back up the position on an issue already taken by the government or by some faction within the government.

The *Royal Commission*, though slow and unwieldy, has still much to be said for it as an investigatory and research device. Typically, it is thorough, takes its time, listens to all points of view at public hearings, commands large resources, and can attract the services of distinguished academics who are customarily allowed to work ahead rather freely without incurring great anxieties about compromising their intellectual integrity. Indeed, a defect of Royal Commissions is often that their research runs a little *too* loose, in the sense that the number of individual projects may be too large in relation to the resources available for coordinating them, backing them up, and subjecting them to periodic expert criticism and review. Perhaps the chief virtue of a Royal Commission is the very thorough exposure which its members receive to the problems under investigation, via contacts with the research staff and public hearings. Even the tedious repetition of old and specious arguments at public hearings may serve an important purpose: it sometimes does more than anything else to turn rather cautious and conservative commissioners into comparative radicals and innovators.

Still another device is that of making a "special reference" of some problem to the *Economic Council of Canada*. The Council has a considerable research staff and may be given financial leeway to add to this as required, although such additions have thus far been much more modest than the hirings of most Royal Commissions. Over its history to date, the Council has striven hard for consensus, with the loss of sharpness and other compromises that this necessarily entails. Royal Commissions also strive for consensus, sometimes obtaining it and sometimes not. The really critical difference, in my view, is that the commissioners obtain a much more prolonged exposure to the problems under review and have more time to work out their consensus. The 25 part-time members of the Council, by contrast, are busy people who come together for one-day and sometimes two-day meetings perhaps six times a year. However assiduously they do their homework, they cannot be expected to digest, discuss, and eventually sign their names to the same volume of detailed recommendations and other material as typically passes through a Royal Commission. On the other hand, the consensus that they do achieve may sometimes have a special sort of importance as a signal to politicians. If such a mixed body of active people can be persuaded of the need for some policy change, it may well be concluded that the country as a whole is also politically ripe for the change.

Too many times in Canada, problems have been taken up, researched, reported on, and then let drop, when by their very nature they should have been subjected to continuing study as they went on evolving. While the final winding up of a Royal Commission may come as a great relief to the Treasury Board, it is not always a good thing for the country. There is a need for a certain number of more continuing research operations, both private and public, large and small, in the zone between the state and academe.

The Bias of Statistics

Returning to my previous effort to give something of an inside picture of how skilled researchers relate to Government, I find it necessary to say a little about some of the things that are done with statistics. People of action tend to prefer hard facts to theories, and force of circumstance, together with the self-selective process, make most senior government decision-makers people of action, even though in their younger days they may have exhibited more reflective and speculative tendencies. In Parliament, and the media too, when you have a tough, rational case to make against strong and emotional opposition, hard facts will generally serve you better than theories. Canadians are a pragmatic folk who enjoy putting down theories.

Against this prevailing prejudice, it must be argued that hard facts are often neither as hard nor as relevant as they seem, and that statistics uninformed by theory are rarely very meaningful. To make a statistical time-series really sit up and talk, one must generally associate it with other series and fit into a model of some kind—preferably an explicit model. When people of action excoriate theory but read great meaning into statistics, it usually means merely that they are using an implicit model—a battered and distorted legacy, perhaps, of that defunct economist of whom Keynes wrote.[4]

Statistics are widely used in government, but also widely misused. They tend to be proliferated second-hand in large and irrelevant quantities, without the degree of processing and analytical structuring that would be necessary to make them into the argumentative props that they purport to be. Also, of course, they can be the subject of crude political games, such as wasting the time of Parliament and the channel capacity of the media in a dispute over which is the best statistical measure of unemployment in Canada that was definitely settled nearly twenty years ago. At a more sophisticated level, display of statistics can be an effective ploy in the game of "showing knowledgeable." Further than this, a command over the recent wiggles in a wide range of time-series may seem to many decision-makers to reduce their uncertainty appreciably. But used in this way, without a good, explicit model, the numbers can be false friends indeed, swamping the mind with an undifferentiated mass of relevant and irrelevant detail.

The development of modern, quantitative economics, wherein statistics are organized into explicit models, and the computer is exploited, and theory is tested against the statistical record, would seem to offer important advantages, appealing to the strong pragmatic streak of people of action while at the same time showing them better ways to use statistics. And, in fact, it does appear that the trend to quantification has on balance improved the status and influence of economic research in government policy-making. For example, few finance ministers are going to fail to be interested in an apparatus that for the first time makes possible a systematic pre-testing of some of the effects of a wide range of tax alternatives.

Yet the urge to quantify, even on the new, systematic basis, with theory built in, has persisting dangers. Chiefly, it imparts a strong bias in research effort towards problem areas where the data is good and where the apparently applicable theory can be readily tested empirically. But these may not be the highest-priority problem areas of the society. Except by lucky accident or unusual forethought on the part of some statistician, a new problem is almost by definition a problem for which fully relevant data does not exist. It must be remembered that statistics are by and large a response to problems, that the construction of a good statistical time-series is a time-consuming act of capital investment on which much of the social payoff only occurs after some years, and that, as a consequence, a country's statistical system is among other things a record of problems thought to be important in the past. Many of these problems will, of course, have continued to be important into the present, and data developed in response to old problems may with a little ingenuity be employed to some effect against new ones. But there are bound to be gaps. Much of the current criticism of GNP and other national accounting measures really amounts to a complaint that the figures do not adequately illuminate problems that they were not, or at least not primarily, designed to illuminate. It is hardly surprising that statistical series, one of whose prime original purposes was to assist in the development of policies to mitigate the short-term business cycle of macro-

economics booms and depressions (a purpose which they still serve admirably), should be less than ideal, at least as they stand, for the analysis of problems of pollution and the detailed separation of economic "bads" from "goods." That a notion should ever have been propagated that GNP was not just a useful tool for the analysis of certain problems, but something approaching a master-measure of human happiness is, of course, deplorable, but it was not for the most part a notion put about by national income statisticians and others who really knew the figures.

It is important, then, that as soon as a new problem has been reasonably well identified, the development of new statistical series relating to it be put in hand promptly. But the production of usable runs of data may well take much time, and meanwhile policy must usually be made. It is likely to be better policy if there is a research input, the research in this instance consisting of a thorough-going theoretical analysis of the problem and a careful use of such existing data as constitutes valid, indirect evidence. If the problem should not be recast to fit the available theory, neither should it be recast to fit the available data.

What it seems to come down to is that research people must be persuaded to seek out and investigate important new problems, even if they cannot for the time being employ against them the full range of sophisticated analytical methods, and that people of action must be persuaded that inputs of theory—even theory that is at least temporarily incapable of being empirically tested—can do much to improve policy. Some of Enthoven's sophomore economic theory, referred to earlier, can at least avoid some of the more egregious inconsistencies and other counter-productive features that too often characterize policy when it moves into new areas.

Conclusion—Proposals

The main burden of the foregoing discussion has been that the findings of social science research do not flow easily and simply into government policy, but make their way through a variety of channels, many of which wind upwards through complex institutional hierarchies. Moreover, on their way up, research findings have to compete with numerous other informational inputs. How can their chances of survival in reasonably undistorted and persuasive form be enhanced? A number of proposals to this end form the conclusion of this paper. They are not, most of them, particularly new or original proposals—some indeed have already been implemented to a degree in certain parts of the federal government. But the trend which I hope they represent is well worth encouraging, and they are duly set forth here.

The primary purpose of the proposals is really two-fold. They seek to improve the status and influence of social science research within government, while at the same time improving knowledge of government and its problems within the research community. Most of them focus on the organization, mobility, and retraining of skilled people, reflecting a view that this is where the big immediate gains are to be made. Among other things, they aim at bringing frontiersmen, bridge-men, and senior decision-makers together more frequently and for longer periods, in a meaningful and stimulating working environment. An underlying hypothesis is that people trained in social science research are a scarce resource, the use of which could be better economized in the best sense of that word, with resultant benefits to the public and greater satisfaction to the individuals involved.

The modest range of the proposals should perhaps be emphasized. They flow out of a single-department model and fall very far short of a scheme to provide the research base for improved governmental planning in Canada. For that, we would need a much bigger model, embracing a number of different departments and agencies, and we would find it very difficult to avoid considering the substantive content of major policy goals. However, I do think that what is put forward here represents a necessary pre-condition to large-scale planning exercises. Most schemes for improved research-based planning by governments seem to me to give insufficient attention to the development and organization of the necessary cadres of skilled people.

The proposals follow:

(1) Where a government entity wishes to make fairly extensive use of the expertise of an outside academic or other economist, the best alternative should normally be considered that of obtaining his full-time services on a leave-of-absence basis for at least one year. This gives the entity the maximum opportunity to pick his brains thoroughly, while giving *him* the maximum opportunity to learn all about the entity—what it does, what its problems are. He may also, in the process, make a useful long-term investment in knowledge of other governmental entities with which he comes in contact during his leave of absence. The next best alternative is to bring the outside expert to Ottawa for a summer, or better still, for a couple of successive summers with some shorter visits in between. Finally, if he must be used but cannot be dragged away from his university for more than short periods, every effort should be made

to give him systematic guidance, counselling, and criticism. An appreciable part of the time of one or more persons in Ottawa should be reserved for looking after him, checking on his progress, and helping to clear up his statistical problems at the Dominion Bureau of Statistics. One very useful device is to have him submit a progress report or two, have another outside expert plus some inside experts go over the report critically, and then invite him to Ottawa for a one-day discussion with all interested parties present.

(2) Government entities should be generous with educational leave and in letting out their research people for extended service with Royal Commissions, task forces, etc. They should also consider letting suitably qualified people out to teach for a year—sometimes, perhaps, in exchange for academics who are being brought to Ottawa on leave of absence.

(3) Following the excellent example of the Bank of Canada econometrics unit, government entities should publish more material for the use of outside social scientists. Some of this material should be in man-readable form, some in machine-readable form, and some in both. There should not be an excessive fear of publishing a fair amount of rather pedestrian but solidly factual stuff; it is surprising how useful this can be. When one moves to the outside after many years on the inside, one acquires a whole new appreciation of such publications as the *Canada Year Book*.

(4) Government entities that have worked hard to make generally available an increasing amount of machine-readable data, programs, etc., to social scientists should not get discouraged at the relatively slow response thus far. There are all kinds of start-up problems. But the demand will come.

(5) Notwithstanding the existing range of institutions, there increasingly seems to be a place in Canadian social science for bodies like the Brookings Institution and the National Institute of Economic and Social Research—policy-oriented research bodies which are at the same time sufficiently removed from policy-making that they can effectively and independently carry through longer-term projects. Such bodies can look at policy problems more deeply and sometimes in advance of their appearance at the political level; they can assemble critical masses of research resources; they can be a place where public servants and academics on temporary leave can work together fruitfully with permanent staff. Over part of their publication programs, such bodies may also render very useful service as high-grade popularizers. Another related function that they or some other institution can perform is that of suppliers of hard-hitting, well-informed, and unquestionably independent comments on current economic developments. This would have been invaluable over the last two or three years. A good test of independence occurs when government and noteworthy non-governmental figures talk economic nonsense, as they sometimes do—the institution unhesitatingly points this out, feeling itself to have a special responsibility never to let incomplete facts or bad logic and analysis pass unchallenged.

(6) Existing programs for retraining and intellectually refreshing middle and senior management in the public service should be reinforced and extended. To be fully effective, they should be generalized over a wide range of people and made virtually compulsory over that range, so that nobody need feel that he is being shunted into a training program largely because the government cannot really think of anything more useful to do with him. The courses should among other things try to give students with original educational backgrounds in social science a thorough survey of what has happened in their disciplines since they were last in school and at the same time reassure them that some of the older things that they half-remembered are still valid and useful.

(7) Similarly, a systematic program of compulsory, short-term people-exchanges between operating and research divisions of government entities might do something to improve mutual understanding and eventually bring about a situation where social science inputs are both more knowledgeable and more highly valued.

(8) People with social science training should also move about more readily *between* government departments and other entities. This is hard to arrange, since any given entity has a natural tendency to hold on to good people. It really requires somebody or something analogous to the late Clifford Clark, who, operating from a position of great power and prestige, and gifted with a deep concern for the welfare of the public service as a whole, operated a sort of one-man, multi-departmental career development service for bright people.

(9) In order to speed up the development of new social science statistics responding to new problems, it might be well to move slightly away from the present, rather highly-centralized institutional organization of statistics in Canada. This should not be interpreted as a criticism of DBS, whose record in statistical research and the development of new series

can be strongly defended, but only as a recognition that DBS is a large, governmental organization with very high statistical standards and a need to relate carefully the possible desirability of new series to the overall resource allocation of the Bureau. Other governmental and non-governmental organizations might in some instances be able to move faster in the pioneering of new series relating to new problems in which they had a particular interest. Once again, this already happens to some degree; but it might be further encouraged. There is, of course, a limit to the "hairiness" that can be tolerated even in very new and experimental data, and it would be desirable in many cases for DBS experts to act as developmental consultants—the more so since it would be appropriate for many new series to be transferred to the Bureau's jurisdiction once they had become well-established. Even as new statistics become more relevant, old statistics become less so. I have sometimes wondered whether it might not be a good thing to form some outside advisory group that could strengthen the DBS's hand in killing off some of its old statistics, so that more of its resources would be available for the development of new ones. Maintaining an individual old series may require relatively few resources, but in aggregate the demands imposed by obsolete series may come to a considerable load. There are, unfortunately, few series which do not have associated with them some vested interests on both the consumer and the producer sides. It is just here that an outside opinion might be useful in helping to dispose of old series which are clearly likely to have little value either in the present or the future. For example, while Western Canada is not the best place to ask this, is it at least possible that the great transformations that have occurred in Canadian agriculture have rendered obsolete some of the agricultural statistics that are now published?

(10) The point was made earlier that not every frontiersman converts readily into a good bridge-man, and still less may he in the longer run convert into a good decision-maker and mandarin. The promotion structure of the public service should recognize this more explicitly. At present, promotion in recognition of merit almost invariably involves more administrative duties, even though the chief merits of the individual involved lie elsewhere than in administration. More might be done to create a dual promotion stream, with opportunity for suitable persons to rise in rank and pay as senior researchers. The net effect of this might well be a better quality of both research and administration within the public service and a better status and influence for research.

The ultimate desirable enlargement of the model presented here would consist of its expansion into an information model of the whole political process, complete with a complex of feedback loops between decision-makers and the general public. Ideally, in such a model, the role of social science researchers and other experts would be that of "resource persons" participating in a process of continuous discussion. Neither they, nor those who have here been referred to as "decision-makers," would in fact have any absolute power of decision. What they would have would be knowledge and influence, the extent of the latter being dependent on their ability to communicate the former and inject it persuasively into a broad, interactive determination of social goals and the means of reaching such goals. But this is becoming very idealistic indeed. The more modest purpose here has been the improvement of a small sector only of the total communications network.

On a final, informal note, might I urge the following on all "outside"—i.e., extra-governmental—social scientists? If you can act as your own popularizer and bridge-man, it is very much in the public interest that you do so. Don't just fling down a system of equations or some other esoteric production and say, "Take it or leave it. If you don't take it, you're stupid." Sometimes governments are stupid, sometimes just preoccupied and overworked. It pays to advertise. Also, processing your own work into more accessible and operational form can sometimes turn out to be quite instructive.

Upon further reflection, the above might be good advice to some "inside" social scientists as well!

Footnotes

[1] It is useless for the reader to seek here a *roman-à-clef*. The model is not only simplified, but generalized and abstracted. The personalities involved are archetypes—not real individuals. The archetypes are treated as the recurring human result of age and of a system and structure.

[2] I am indebted to George Post for this point, which squares with my own experience.

[3] One of the many simplifications of the model is the grouping of people with research skills into a single division. In practice, such people may be found, not only in a research unit designated as such, but also in operating divisions, in high-level advisory and policy-planning groups, etc.

"'Practical men, who believe themselves to be quite exempt from any intellectual influences, are usually the slaves of some defunct economist." Keynes (1936: 383).

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Utilization of Social Research: The Alberta Case *

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MARCH 1971

Utilization of Social Research: The Alberta Case *

Some General Considerations

The relationship between the people who promote research and those who utilize its results has not been properly investigated, although it may be of crucial importance not only for the public image of social research, but also for its future. It is at present a common complaint that social research is losing its feeling for reality and becoming merely a scholastic game for professionals.

In order to study the relationship it would be helpful to look for specific cases which may provide evidence of problems on the border between the world of social science and the world of social practice.

Since the Province of Alberta is the subject of our study, it seems reasonable to look at the situation in Alberta. Three large urban communities in the province have universities, established in order not only to train students and accumulate knowledge, but also, at least to some extent, to serve their respective communities. The existence of these three universities and a number of smaller research centres poses certain problems: How do they promote work in the field of social research? How much mutual understanding may be counted on? What are the reciprocal images of the people concerned? How do the basic management principles of stimulation and coordination apply to the managing of social research? How may a policy by established to promote research valid in terms of basic local needs? How may the waste of human and mental resources in unnecessary research be avoided?

The needs implied by these problems are all local and immediate, and the development of universities and research centres in Alberta or anywhere else should not be evaluated only in terms of what is needed here and now. A broader, universalistic orientation in dealing with local research resources is a necessity. However, in order to justify the expense, it seems necessary to develop a research policy which would make social research and social researchers really useful on the provincial and local levels as well.

The pursuit of social goals in Alberta, as in other Canadian provinces, is becoming a responsibility of provincial programs and services. One of the most important tasks of social research on the provincial scale is the establishment of a scientific basis for pinpointing social indicators dealing with health and illness, social mobility, living conditions, income and poverty, public order and safety, learning, science and art, participation and alienation. (U.S. Department of Health, Education, and Welfare, 1969).

The establishment of such a basis depends to a large extent on the development of an "organizational intelligence," consisting of appropriate questions, insights, hypotheses, and evidence. Such an intelligence, combined with successful completion of the tasks of goal setting, innovation, and control, is the stuff of complex organizations. And, such an intelligence rests with people—intelligence experts, to give them a label, however unsatisfactory.

The position of such intelligence experts in relation to organizational leaders, and the intentions and qualities of those leaders, are crucial in determining the extent to which the intelligence is used. If the leaders see its main function as "back-stopping," or "window-dressing," the position of the intelligence experts is weak. In some organizations under external control, the applications of research data are not really useful because the local leadership has a very limited formal power.

Intelligence experts must guard against several possible contradictions within their practice. Although they should contribute to the critical evaluation of current policy, they may become biased and concentrate their whole effort on inventing slogans to reinforce the adherence of policy-makers to myths, "thereby blinding the vision and reducing the number of recognized options." (Wilensky, 1967:8-9). They may lack the insight and intellectual courage necessary to rid themselves of preconceptions. They may fall prey to the ambition to gain power by developing the bureaucratic apparatus of intelligence, to the detriment of their original goals. The tensions among their various branches may become the basic issue. The means of dealing with issues may become too sophisticated and, therefore, ineffective in simple situations.

The relationship of social sciences to those who must use the results of their organizational intelligence has inherent in it a number of tasks: the appropriate translation of scientific generalizations into practical directives (Podgorecki, 1969); finding enough fields for the practical applications of scientific concepts (Dean, 1965; Bennis, 1964; Shostak, 1966; Lazarsfeld, 1967); finding something common between the language of politics and the language of social research (Machiavelli; Butterfield, 1962; Mackenzie, 1967; Merton, 1949); overcoming prejudices and distorted views about professional experts, particularly scientists (Shils, 1956); finding ways of convincing people in power of the utility of

the expert knowledge (Milbrath, 1963; Brown, 1963); making the relationship between the managerial elite and the experts fully meaningful. (Gross, 1966; Burham, 1941; Young, 1958; Simirenko, 1966; Sjoberg, 1967; Gouldner, 1965).

The problem of furnishing reasonable advice on the translation of goals into reality becomes especially complicated in the case of modern bureaucracies. [1]It is necessary first to formulate empirical hypotheses in such a way that they can be translated meaningfully into teleological directives. An attempt is now being made to create socio-technique as "an applied science which is concerned with the attainment of desired social effects taking known, tested, and accepted relationships and evaluations into account. Sociotechnique is an applied (or practical) social science which serves to inform the practitioner on how to find effective means to realize intended social goals when a given set of values is accepted and a given body of tested propositions describing human behavior is accessible." (Podgorecki, 1969)

The relationship of the clinical social engineer to the classical social engineer is basic to the position of social scientists with respect to the organizations which should use their advice, as is the degree to which direct intervention in social reality may be acceptable. Here, the difference in values between the sponsor and the social engineer must be considered: "Many persons and agencies can wield the knife of sociotechnique; however, their values do not and should not influence the sharpness of the knife (akin to the efficiency of the techniques of social engineering), nor the area where the knife is to be used." (Podgorecki, 1969; Merton, 1949; Popper, 1945)

To deal with the social technology of organizations, it seems necessary to distinguish between general praxeological principles valid for all kinds of task-oriented systems (Kotarbinski, 1965) and principles of middle range or lower range. "The consultant role is perhaps the most effective tool the sociologist has to bring about desired social change." (Shostak, 1966: 6) However, the role of a consultant may be defined in a number of ways. To make him really efficient, it is necessary to establish favorable conditions, such as power to concentrate effort on important issues, [2]the relative independence of the social engineer from external pressures, etc. An important condition is adequacy of sociotechnological directives to the particular situation of practical action. General rules must be translated into directives relating directly to the concrete conditions of action, with distinction made between various levels of applications, avoiding the common error of applying directly rules from a higher level to situations on a lower level.

One final consideration: the growing importance of organizational intelligence in the age of bureaucracy, managerial revolution, and meritocracy brings into the open the vital issues of dysfunctions. (Merton, 1936) Is the growth of organizational intelligence really dangerous for democracy? What is the difference between an organizational intelligence which serves the people and such an intelligence which leads to totalitarianism? How can modern techniques of research and persuasion be used to strengthen democracy instead of weakening it? To answer such questions it is necessary to distinguish among various patterns of intelligence related to specific institutions, types of social and political cultures, and types of social engineering, i.e., manipulative engineering against engineering for social maturity of systems. (Matejko, 1970)

The Alberta Case

It is necessary at the outset to determine what is needed in the Province of Alberta in terms of research. Professional preferences should not be treated as the only source of knowledge in this respect. The growth of social services provided to citizens of the province brings to light several vital questions which need a substantial amount of research to be answered adequately. And the growing demand for more understanding of social research and its goals cannot be satisfied completely in the existing framework of mutual relations between social science and social practice. Several obstacles to mutual communication leave the effective contribution of social research in doubt.

To locate and identify problems of this nature, a survey was initiated in 1970 on the potential for, and the utilization of, social research in Alberta. Questionnaires were sent to agencies whose activities should create an interest in social research. A register of social scientists in the province was compiled, and potential clients of social research in the province were contacted about their view of the present picture in the province and about their own needs.

The findings of the questionnaire are summarized in Table I which indicates that the respondents recognize the fact that their agencies face some social problems and that social sciences may provide some help in this respect.

A number of observations can be made from the survey about the social research picture in Alberta.

They are discussed with the conviction that the same situation prevails elsewhere and that these observations, and the following conclusions, about Alberta can be projected beyond this province.

TABLE 1

Summary of Answers Provided by Actual or Potential Clients of Social Research (in percent, n = 106)

Problem	No Answer	Negative Answer	Positive Answer			Together
			Some	Much	Very Much	
Recognition of the fact that some social problems do appear in a particular agency	6	3	20	33	38	100
Recognition of the fact that orientation in social sciences is needed when dealing with problems faced by a particular agency	17	2	14	53	14	100
Reading of social research publications	18	7	19	34	22	100
Utilization of those publications in policy-making	21	19	37	20	3	100
Initiation of social research studies	13	26	33	18	10	100
Satisfaction with contacts between one's own agency and social researchers	25	4	15	44	12	100
Recognition that social research is really necessary for a particular agency	8	4	29	36	23	100
Expectation that social research, as it is now, may provide a real help in solving problems of a particular agency	7	4	24	45	20	100
Recognition of the fact that it is difficult to implement research which would have practical implications for the agency	32	1	4	45	18	100
Ability of a particular agency to facilitate research	41	2	16	31	10	100
Perspective that social research will be needed by a particular agency in the long run	21	2	4	23	50	100
Present contact with the academic world	39	11	29	14	7	100

Observations About the Social Sciences in Alberta

1. There is a serious lack of proper channels through which support for research (finances, consultation, etc.) can be gained, a situation which prevails outside the three universities, where such support is available.

An organization apart from the universities, perhaps within the government, might help to fill this gap, and to coordinate non-university research efforts.

2. The increasing organizational complexity of the field of social services creates a special need for follow-up studies to provide reliable criteria for the measurement of effectiveness of various programs in social welfare, a field in which this complexity and the problems it causes are especially evident.

3. The problem of organizational effectiveness is of great importance to all agencies, and especially relevant to the level of information among the public on the goals of agencies and their fulfillment, to the satisfaction with the work of agencies on the part of clients, and to the channels of communication and the mutual interdependence of various agencies.

4. Organizational effectiveness is related to the problem of conservatism on the part of the governmental bureaucracy not exposed to market competition. It might be advisable to build into the organizational structure of governmental institutions

such individuals or groups, who could focus their efforts on continuous evaluation of organizational performance, including the prevention of the alienation of clients and the promotion of innovations. Such built-in controls might possibly be reduced in effectiveness by the tendency of agencies to limit the intrusion of social scientists into their functioning.

5. The under-utilization of existing staff resources for the promotion of locally valid research results in a substantial lack of contact between practitioners and the local centres of research. And, the problems of the adaptation of agencies to the external environment are often emphasized by people in the agencies. These problems become of special importance when the agency has its own middle-men to reconcile its interests with the broader interests of the community.

6. An awareness of too much "red tape" in social agencies stimulates an interest in such research as would reveal the extent of bureaucratization and the resulting breakdown of communication. Preconditions established by agencies form an obstacle course for those needing help, and there is a feeling that government supervision of social welfare is often more concerned with regulations than with helping people.

There is a lack of cooperation between supervising bodies and within social agencies. Social workers with unmanageable case loads are often not properly aware of the services offered in their own and other agencies. The public is not kept informed about available services, which may themselves be hampered by "mechanical gaps."

7. The insularity of individual agencies creates barriers to communication among themselves and with the public. This situation naturally reduces effectiveness. Public sectors show a tendency to swamp and attempt to incorporate private agencies.

8. Financial pressures have created austerity situations, and many agencies suffer from insufficient staffing.

9. The establishment of priorities is made difficult by the demands of solving practical problems dictated by the current needs of clients. The lack of an informal network of information-yielding contacts is an important factor contributing to the practical problems encountered in establishing communication between social scientists and potential or actual clients. Researchers apparently do not pay enough attention to developing meaningful contact with people responsible for practical action because they assume that it is not possible to learn from them.

10. Communication between academics and agencies does not work well enough. The isolation of academics leads to irrelevant research and work in problem areas having little relation to actual problems. Meaningful communication between originators and producers of research and potential clients must be based both on the subject matter of the research and on ways in which the research may be implemented and applied.

11. Many users of research have difficulty in translating terms used by social scientists to a level allowing them to use pertinent studies. This problem could prevent social research from accomplishing what it sets out to do. Indeed, many agencies seem quite familiar with the concept of social research, but they find it difficult to apply it to their own field of activity.

12. Frequently, research projects, after wide publicity and the generation of much enthusiasm, will lack the budget for necessary follow-up, and new frustrations are created.

13. The need for social research and social planning at the city level is evident. The fast growth of the urban population in Alberta (from 46 percent of the total population in 1951 to 68 percent in 1966) contributes to problems related to the integration of new city dwellers. Research could help in establishing definite neighborhood boundaries, in providing information about urban services, in determining alternatives to the present system of social assistance, in measuring the effectiveness of various innovations, in upgrading and retraining, etc. Of primary importance is the establishment of a hierarchy of priorities in dealing with problems in this area.

14. The economic and social organization of various health agencies run or supported by the government presents a very attractive topic for research. Many problems appeared, including the professional relationship between medical staff in hospitals and social workers working with some patients, follow-up procedures with patients requiring social assistance, treatment of permanently handicapped persons after leaving the hospital, etc.

15. There is a lack of research in Alberta dealing with law enforcement, with a transfer and diffusion of responsibility from law enforcement agencies to academics who are not anxious to accept the responsibility.

16. The purpose and utility of social research is apparently not understood or appreciated by the mass media, who seem to view social research as something conducted in universities, with little rela-

tionship to reality. While responsibility for this lack of awareness lies on both sides, the necessary dissemination of research findings to the public suffers.

17. It is a fact that people move constantly from one place to another. According to survey data from 1966, 40 percent of those in Canada between the ages of 25 and 44 with university degrees obtained their degrees outside the province in which they lived. The fact that provinces, including Alberta, are able to retain only a portion of their Ph.D. recipients creates difficulties.

Three summary observations may be made:

- —A stronger link and much more effective communication are required between researchers within and outside the academic community and decision-making organizations in the community.
- —There is a need, perhaps an urgent need, to develop the technique of translating research data into practical objectives. Some vehicle or forum is necessary for the discussion of research findings and practices from the practitioner's point of view.
- —More contact with and experience in research is required by social workers and others who are required to put research into practice, but who, in many cases, lack the background to do so effectively.

Conclusions and Possible Solutions

The implementation of social research data is a complicated problem. *Who* should introduce changes in social and organizational reality, and *why* these changes should be made are often political issues. Social scientists still do not represent the sort of pressure group which influences the political decision-making process. Therefore, decision-makers in organizations do not need to take social science into consideration. "Some reject social science knowledge even when they agree with it, when the resources needed to attain the indicated solutions are too demanding." (National Science Foundation, 1969; Lyons, 1969)

On the other hand, social scientists often do not offer social services which could be fully appreciated by men of practice. "In many instances the social sciences provide accurate descriptions of predictions of events, but no solution to the problem. It is too easy to reject such relevant knowledge out of hand, in the belief that it is simply troublesome when it provides no answers." (National Science Foundation, 1969: 12)

One possible solution to the question of irrelevancy is to extend the practice of employing social scientists in government and in other organizations. For example, the Special Commission on the Social Sciences in the U.S. recommends "strengthening and extension of present practices of employment of social scientists in the federal government, and specifically recommends that an extensive program of periodic leaves of one academic term at full salary (comparable to academic sabbatical leaves) be instituted to enable professional employees to bring themselves up-to-date with the very rapid developments of their own and related disciplines, either by means of specially designed refresher courses, or by working on research of their own choosing." (National Science Foundation, 1969: 14-15)

The proliferation of demands for research creates a need to reconsider relationships between governmental bodies, as sponsors of research, and the academic community. (Organisation for Economic Cooperation and Development, 1969) "Big science" and the growing competition for scarce resources among scientists cultivating the different disciplines make corporate action on the part of scientists more important." (Doern, 1970) It becomes evident that universities so far are not prepared to handle properly problems of research when it comes to some large scale projects. "It is doubtful whether any single university, or even the universities collectively, have any machinery to coordinate plans or resolve internal conflicts of an interdepartmental nature and to represent their interest in the discussion. The universities in Canada have no research policies and have lacked the institutional arrangements to arrive at such policies." (*Ibid:* 374)

Contact between social researchers and social practitioners needs not only more careful insight, but also much more of an organizational effort in terms of stimulation and imaginative coordination. There is a growing need not only for the qualified "bridgeman" mentioned by Professor McQueen, but also for the *social technology* of applying research. It is a basic misunderstanding that mere financial support of research is enough to improve the situation. However, an undiscriminated proliferation of social research has its serious dysfunction in terms of growing confusion and overloading of communication channels. If the relationship between the various streams of information is a competitive one—as McQueen says—then of course it would be unwise to lower the competitive power of social research by allowing it to become inflationary.

What is needed now is more discussion of priorities in social research and the social technology of implementing research results. In both cases, one should take into consideration the experience of other societies, first the U.S., to avoid repetition of

errors committed by others.

The activities of various voluntary and governmental agencies may be stimulated and improved by social research. However, in order to achieve this stimulation and improvement, it is necessary to bring researchers much closer to the realm of practice. Unless they become familiar with and even to some extent committed to the practical goals of the agencies, it will not be possible for social scientists effectively to cross the bridge. There is a vital need for both groups—social scientists and social practitioners—to meet more often in order to develop intellectual cross-fertilization. Such meetings should be informal to allow vigorous debate.

The vital question of involving social scientists in the service of practical needs could be solved, at least in part, by formulating properly issues to be investigated by social scientists. More information is needed, and practitioners should be exposed to the constant flow of stimuli close to their own needs and interests, through the establishment of adequate research centres and local periodicals in the field of social science.

Stimulation of practitioners with knowledge from the social sciences should be accompanied by attempts to recruit their active participation in promoting research. Many research projects would gain if preceded by an examination of existing practical experience. In many cases, practitioners could join survey programs with researchers, a practice which could be especially valuable in fields in which the insight of researchers would be limited by the difficulty of gaining data. Such mutual contributions to mutual problems could lead to better understanding and stimulation.

Footnotes

*Revision of the paper presented at the Conference on Social Science Research and Social Policy.

[1]A possible answer to the question would be to make more use of specially designed management or brokerage agencies which would develop the partnership between governmental bodies and various contractors more able than a governmental bureaucracy to reach some concrete objectives. The gradual development of such agencies would create a great demand among others also for sociologists. In other words, sociological research and consultation may contribute substantially to the improvement of educational output and the productivity of the educational system. W. G. Manning suggests the necessity to clarify the objectives of the educational system, improve public relations, use personnel more effectively, raise the competence and the prestige of the teacher, develop curricula to achieve the objectives of the school system, improve the process of instruction, support educational research and development, improve the school management, insist on cost efficiency, etc. W. G. Manning, 1970. *Toward a Breakthrough in Education, A Systems Approach to Educational Productivity.* Edmonton: M. & M. Systems Research Ltd., pp. 43-45. See also *Requests for Proposals and Social Contracts.* Paper 1. "A Strategy for Organizing Resources to Achieve Social Goals," Paper 2. "A Strategy to Advance the Role of Private Enterprise in Canada." Edmonton: M. & M. Systems Research Ltd., 1970.

[2]"It happens quite often with social research that leaders of a particular organization select from research results only the items which suit them most. "Where our findings coincided, action was taken. When we pointed to other trouble spots than those originally perceived, the organization tended to ignore them. For whatever reason, action research in this type of setting may simply be a way of legitimatizing the preconceptions of organizational *leaders.*" R. H. Hall in A. B. Shostak, (ed.), *op cit.*, p. 38.

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Social Science Research as an Evaluative Instrument for Social Policy*

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MARCH 1971

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"Lecturing on navigation while the ship is going down."
W. H. Auden

Introduction

In his L. T. Hobhouse Memorial Lecture of 1957, Alexander Macbeath posed a question which is fundamental to all work in the social sciences — can social policies be rationally tested? I have implicitly answered the question with a cautious affirmative. This has allowed me to proceed to the next question of interest: how can it be done?

However, it would have been unwise to brush aside completely the Macbeath question, so I have commented on it in the first section of the paper; it is helpful to do so in order to establish the legitimacy of my subsequent reflections. Moreover, these prolegomena shall enable me to clarify the idea of social science and to specify the boundaries of social policy.

In a second section, I attempt to differentiate among the many contributions of social science research to policy. My argument is that one cannot separate the regulative from the innovative function in policy-making. This integrated view of policy analysis leads to a redefinition of the role of social science research and entails a refurbishing of the social scientist's tool-box: social science must shift to experimental intelligence as a method. Some even go as far as to claim that institution-building is now at the core of policy-making: policy as the designing and operation of some low-structure permanent institutions supported by some planned non-permanent ones.[1]

In the third section, I take a critical look at the present use of the social sciences in Canada. My claim is that the expertise that many social scientists bring to bear on social policy is largely bogus. Mishan[2] has indicated this very precisely in the use of transport economists and my point is that, to a certain extent, his argument can legitimately be extended to all social scientists.[3] This leads me to speculate on the optimal use of the social sciences for public policy and on the role of the social scientists in the interface with the public administrator.

Social Science and Social Policy

Social scientists have wasted little time on the question of the possibility of a social science. However, this is an issue on which philosophers and epistemologists have speculated at some length. (Granger, 1960: Ch. I) Social scientists have displayed little taste for such transcendental exercises because they are mainly interested in the *de facto* social science rather than in a *de iure* imaginary one. Their tangible social science is a "practice" in the Marxian sense: A set of procedures regarded in Wilson's words (1970: XV), as "the best way we know of investigating" social reality. The possibility of such a "practice" is amply demonstrated by its very existence.

However imperfect the present procedures may be, an archaeology of the social sciences would reveal that, since the nineteenth century, the inexact sciences have become more potent. (Helmer and Rescher, 1960)

From the observation-only stage and the assemblage of information, to the construction of more and more complex classification schemes, to the development of functional relationships, the social sciences have matured. They have developed from empirical sciences into formal ones, from a body of information into a language of problem solution.[4] In the words of Rapoport (1965: 208), "the making of social science . . . depends on the creation of scientific models to replace the metaphors in which so much discourse about social matters is still couched." The construction of such models has been over recent years the activity of most social scientists and they have moved steadily away from concern with superficial analogies to interest in more meaningful homologies.[5]

The language-of-problem solution developed by the social scientist is often quite different from the unspecialized language in which citizens and other social actors are couching their problems. This has led to charges of esoteric mannerisms and unreality. To a large extent, these charges are founded. Since Kant, obscurity has acquired some respectability and from the premise that "profound reasoning is difficult to understand," obscurity has often been taken as evidence of profundity. (Medawar, 1967: 9-10)

Social reality is complex: too much concern for strict adherence to all aspects of reality and too much limitation on the development of a specialized language to deal with it may grind the scientific inquiry to a halt. Da Vinci was unable to construct a flying machine because he was trying to reproduce a bird. His planes resembled birds so much they could not fly. A bird with fixed wings and a propelling beak proved a much better "simulator" of the "real" thing than a straight copy.

The social scientist has to develop a set of concepts

and categories which will enable him to proceed efficiently with his model-building. In the same way that Eskimos, in order to survive, require a wide variety of words to identify different types of snow, social scientists need an elaborate operational vocabulary for their problem-solving.

The social scientist's activity amounts to the assemblage of information and the development of concepts enabling him to produce useful classifications and functional relations. The essence of model-building is a two-stage process by which the social scientist *partitions* the reality he observes into fragments and *constructs relationships* among these sub-units in a manner which will provide better "simulacra" of the reality than an exact image, better in the sense that it will simulate better the process of interest. (Barthes, 1963; Paquet, 1964)

In the early days of the development of the social sciences, social scientists used to approach reality as a "total social phenomenon" à la Mauss. They had not freed themselves yet from the traditional cosmologies. However, with the increased specialization of the social inquiries, there has been a fantastic burgeoning of the *ceteris paribus* clause and a shift from the study of structure of society to the study of functioning of some parts of it. This was the major trend until the last decade: the modelling process was emphasizing *continuity* based on the beliefs in the permanence of certain functions, on the "enchaînement" of conflicts, and on a certain assumed stability of the fabric of significations making up reality. (Foucault, 1966: Ch. 9) But, with the speeding up of technological change, and the emergence of a society which is essentially temporary (Bennis and Slater, 1968), modelling has had to focus on *discontinuity*. Because of the urgency of the times, the refurbished confidence of social scientists and their stronger basis in the 1970s to talk about the potentialities of applied social science, attention has shifted away from social functioning back to the social structure, with a view to using knowledge to effect organizational and institutional change. (Bennis, 1964-1965)

In this new context, the social scientist is not merely an observer of the social reality or some sort of diagnostician of malfunctioning in the system. He is a genuine social engineer designing new organizations and institutions. Social scientists are the *new utopians* (Boguslaw, 1965), whose function it is to redesign the social system in order to facilitate social change in a turbulent environment. (Emery and Trist, 1965)

But, if social scientists are to perform such a task, they need criteria by which to gauge their designing activity. Applied social science is a deliberate and collaborative effort of the researcher and the manager to improve the operations of the system. It is not sufficient for the man of knowledge to proffer a rational solution, for the owl to tell the grasshopper that to avoid the severe pains of winter, he simply has to turn himself into a cricket and hibernate. (Bennis, *et al*, 1961: 3) This does not quite satisfy the client who might legitimately ask how one goes about performing such a metamorphosis.

The interface between the social scientist and the administrator of social policy has changed considerably as both the social sciences and social policy underwent radical changes in the period after World War II. For, as the social scientists turned themselves into social engineers, social policy emerged from the doldrums of the welfare statism to take on a broader and more dynamic meaning.

It has not been easy in the past for social scientists to collaborate with the executants of social policy, for they appear not to be quite clear about the nature of their task. As late as 1965, T. H. Marshall could write "'social policy' is not a technical term with an exact meaning." (Marshall, 1965) Although the exact meaning of "social policy" was unclear, the term was very clearly linked to the broad concept of the welfare state. This meant orientation toward the well-being of those elements in society which were exposed to contingencies mainly in the name of charity and philanthropy. (Frank, 1967) As we moved toward the post-industrial society, a need was felt for going beyond this alleviative approach, and the state redefined its action: its action tended to shift from a strictly passive and alleviative *post factum* approach to active social engineering which would ensure that each citizen gets fully "developed."[6]

This shift in the nature of social policy toward social development (i.e., toward the effective realization of the Service State in which the individual has a right to maturation, learning, and extended adaptability, and in which social policy is directed at removing arrest, retardation, and waste) is now largely accepted and has been promoted by diverse bodies, although usually in a diversity of idioms.[7]

In an attempt to define the boundaries of this new social policy – boundaries which go much beyond charitable gestures and welfare policies but which do not obviously encompass all of public policy – and the common thread that unites all aspects of it, Kenneth Boulding (1970) has used a broad view of social dynamics defined as a three-fold genetic

evolutionary process. It involves a threat system (mainly studied by political scientists), an exchange system (mainly studied by economists), and an integrative system (supposedly studied by sociologists). According to Boulding, any concrete social organization can be regarded as an assemblage of processes of the three types, although the valence of each system may differ significantly from organization to organization.

The common thread that unites all aspects of social policy is the *"integrative system"*: "this includes those aspects of social life that are characterized not so much by exchange in which a *quid* is got for a *quo* as by unilateral transfers that are justified by some kind of appeal to a status, or legitimacy, or identity or community." (Boulding, 1967: 7) The problem is to recognize that "in any situation, there is an optimum degree of integration" *(Ibid.* 8) while acknowledging that we have very little theory of the over-all system and very little information even on its present performance.

We know more about the exchange system than about either the threat or the integrative system. Consequently, we can say more as social scientists about economic policy than about social policy, for instance. But the developmental philosophy provides us first with a useful dynamic norm recognizing the active integrative role of social policy. As long as we were living in T. H. Marshall's universe, where social policy took no exact meaning, it is hardly surprising that we could generate no exact policy suggestions. Now that the notion of social policy is more clearly specified, we should be able to seek models of the "social market" in the same way as we have for the "economic market." Richard Titmuss (1968), in his usual candid way, has already volunteered an eight-point categorization of the task – one that encompasses "not only goal-formulation but administrative processes, historical change, and methods of delivering, measuring and evaluating services and systems" – devolved to social science research in connection with social policy.[5]

Up to now, the dialectics researcher-manager has been cluttered with ill-specified conceptions from both sides. If this were the main reason for a poor inter-creation process between social scientists and social administrators, we might now have some hopes. But, this assumes that social scientists are able to make a positive contribution to the operations of social policy and that what has prevented such a contribution up to now are mainly what Guetzkow (1959) calls "conversion barriers."

Social Science Research as an Evaluative Instrument

In what ways can social science research be of help to the social policy administrator, given the present state of the art in the social sciences and the elusiveness of the social policy situation? One could imagine a variety of types of research inputs and a diversity of devices to plug them into the policy process. Each such cross-classification would correspond to a specific theory of policy. Since it is not our purpose to provide a primer on praxeology, we will be satisfied to identify the major areas of concern of the policy-maker without getting involved in an extensive discussion of the many ways in which these could be differently labeled or structured into different policy systems.

One may identify the major areas of concern of the policy-maker under four headings,[6] as per Wilensky (1967):

(1) goal setting
(2) control
(3) innovation
(4) intelligence

While the traditional approach to policy-making has largely emphasized the first two components, much of the recent literature has questioned the usefulness of such a focus and has tended to put more emphasis on the latter two components. It would appear that the social scientists have come to realize that, for the time being at least, this second approach is more to the point, for two interrelated reasons: the social sciences can neither properly reduce social policy to well-structured problems amenable to simple control procedure nor provide a language of organizational purposefulness which could help the policy-maker to clarify his goals *ex ante*. This makes the traditional approach somewhat less than helpful. However, an alternative approach based on intelligence and innovation has not really been fully spelled out yet. We suggest that enough elements are now available for us to attempt a preliminary synthesis. This would constitute a cybernetic theory of policy originating with the intelligence function, mainly process-oriented, and providing the goal setting (if any) *post factum*. The cornerstone of this theory of policy would be the notion of norm-holding.[7]

If social development can be regarded as the norm, i.e., the broad direction of social policy, we are still very far from a set of precise targets which could be inserted into a function to be maximized subject to constraints. The conflicts between the utopian cast of mind of the philosopher-king and the piecemeal experimentation à la Popper-Lindblom are still

unresolved.[11] It would therefore be premature to attempt a translation of this "sense of direction" into a set of specific goals which could then be subjected to operations-research type controls for their effective realization.[12]

A more realistic approach starts from the assumption that the problem of social policy is still largely ill-structured, recognizes that the executants of social policy have to operate in a rather turbulent environment, and seeks to define a philosophy of social policy which steers a course between mindless incrementalism and premature utopianism. This requires from the social sciences something more than the present set of operations-research type tools which postulate the existence of well-defined goals and well-specified constraints. If the problem is ill-defined, it requires a research strategy adapted to it.[13]

Between the *well-structured problem* requiring only the working out of an algorithm and the use of operations research methods, and the *completely unstructured problem* based on the heuristic method of the classical social scientist drawing mainly on intuition, each case on its own merit, there is the whole field of the *ill-structured* problems which calls for "a compromise between the well-structured methods of OR and the unstructured method of the heuristic approach." This is the family of quasi-analytic methods suggested by H. I. Ansoff, (1960).[14]

The quasi-analytic methods are *process-oriented* rather than *outcome-oriented* as are the usual operations-research methods. This is the crucial difference: it emphasizes the continuous feedback between analysis and problem formulation, thus creating a self-correcting theory of organization.

The role of social science research in this context is quite different from that in either of the other methodologies. Indeed, since the problem formulation itself is open, the evaluative function involves the designing of an information system to provide the medium for effective feedback between analysis and problem-formulation. The interplay between norm-setting, goal-setting, course-holding, control on functioning, and organizational and institutional innovations becomes fundamentally dependent on organizational intelligence. The medium of the monitoring device becomes a major factor in the shaping of the message. Instead of the standard surveillance, we are faced with a genuine learning process generating a self-organizing system.[15]

In this dynamic context of ill-structured problems, the notion of evaluation is completely changed. The difference between ends and means, between blueprints and little steps, between policy objectives and the social technology which is set up to pursue them, is blurred, and the social scientists involved in the policy process gain more power and more responsibility.[16]

Instead of a set of goals exogenously determined, the desiderata emerge from the exploration of the feasible: to explore, one *starts with a highly tentative initial formulation* and through a strong feedback mechanism *learns*, and *constructs* a local plausible theory for the phenomena under study while erecting the social technology to steer the system and organize it rather than simply regulating it.[17]

This cybernetic formulation of policy-making is a learning process which hinges on the intelligence function. There is therefore some urgency in the need to refurbish accordingly the concepts and the categories used by social scientists to provide the necessary instruments to operationalize this new primacy of the intelligence function.

The "new social scientist" will require a good knowledge of a new conceptual framework, of new search processes, and of new testing techniques which take a fuller account of the nature of the problem at hand. This is not the place for a complete review of this arsenal, but it might be helpful to review a few of its pieces, if only to fix ideas. We shall say a few words about *transduction*, *experimental utopia*, and *simulation*, all of which are at the core of this revolutionized social science.

Transduction refers to an intellectual operation which differs from induction or deduction and which

> "élabore et construit un objet théorique, un objet possible et cela à partir d'informations portant sur la réalité ainsi que d'une problématique posée par cette réalité. La transduction suppose un feedback incessant entre le cadre conceptuel utilisé et les observations empiriques . . . Elle introduit la rigueur dans l'invention et al connaissance dans l'utopie."[18]

This form of reasoning is congruent with the quasi-analytic method of Ansoff and constitutes a valuable way to incorporate into the thought process the learning dimension which is crucial to the dynamic monitoring we are interested in. Instead of the ordinary model construction and statement of hypotheses, we have a form of reasoning which calls for feedback between problem formulation and analysis from a preliminary and highly tentative point of departure.

This formal procedure is fed, so to speak, by *experimental utopia*. If by utopia we mean the exploration of lateral possibilities (Ruyer, 1950: 9),

experimental utopia refers to

> "l'exploration du possible humain avec l'aide de l'image et de l'imaginaire, accompagnée d'une incessante critique et d'une incessante référence à la problématique donnée dans le réel. L'utopie expérimentale déborde l'usage habituel de l'hypothèse dans les sciences sociales." (Lefebvre, 1961: 192)

This method wishes to invent new forms, but new concrete forms; consequently, it uses variations around the theme defined by reality.[19]

In this process of transduction properly fed by experimental utopia, the cost of exploring and of thinking may be much less than the cost of experimenting. One way to reduce dramatically such costs is to use *simulation techniques*,[20] which allow a research worker to try out his proposed technology on alternative artificially-generated environments or to try out alternative social technologies in a given environment without having to conduct the "real" experiment. Enough experiments have been conducted for us to regard such methods as very reliable approximations.

A compendium of transduction-cum-experimental utopia-cum-simulation would appear to constitute a viable strategy of dynamic monitoring of social policy which accommodates very well the ill-structured nature of the problem, the new trends in the social sciences, and the stronger role of innovation and intelligence in the contribution of social science to economic and social administration. It also leads to a superior approach to these eight tasks of social scientists categorized by Titmuss.

The outcome of such dynamic monitoring will be a rather innovative learning process and an improving system design rather than a static information feedback.[21] This leads one to ponder on the type of redesigning of the present system of social policy which is likely to develop as this new dynamic monitoring becomes prevalent.

We know that the utopian renaissance has led to many forms of approaches: the *formalist approach* (based on the use of models), the *heuristic approach* (based on certain principles guiding actions), the *operating unit approach* (based on carefully selected people and equipment) and the *ad hoc approach* (based on the present situation).[22] In the face of the problems of social policy, my contention is that the formalist approach is really unhelpful, or, if one is really optimistic, premature. The "muddling-through"-cum-"seat-of-the-pants" techniques implied by "ad-hocery" are also rather unhelpful if only because they are "parcellary" and myopic. The choice facing us in "emergent situations" and in a "turbulent environment" is therefore between the heuristic and operating units approach, rooted in a set of principles *or* in a selection of reliable and flexible components. This latter solution clearly limits unduly the evolution of the system and its capacity to deal with a changing environment (if the components' performance is reliable) or limits the controllability of the system (if the components are self-sufficient).[23] Heuristic system design would therefore appear to be *a priori* the preferred approach.

The central task is for the social scientist to erect heuristic designs on the basis of the *developmental principle* which emerged from the discussion of our last section.

Can the social sciences demonstrate that the learning process triggered by such dynamic monitoring will lead to "better" results than those achievable by administrative rules of thumb? Boulding and many others would be willing to state that such is the case and I hold the same view. But since, as Boulding says (1967: 9), "we have very little theory of the overall system and practically no overall apparatus for collecting, processing and feeding back information," and since no good feedback apparatus exists between analysis and problem-formulation in social policy, there is no hope that the present haphazard and *ad hoc* practices will soon disappear.

It may well be that, as a transition strategy, one will have to rely on the operating unit approach: the involvement of key individuals ready to get close to where the action is and interested in putting themselves in a position to get the ear of the prince. Indeed, one might say that this is now current practice. It may turn out to be the least objectionable way to effect those organizational and institutional changes necessary to improve social policy. While the involvement of social scientists as entrepreneurs in social change is not without creating some unease among the liberal-minded intelligentsia, it may turn out to be the only viable and effective strategy while we organize better ways for citizens to participate in social development. If the alternative is an organizational standstill in a period of accelerating change, I, for one, would accept this option as a second-best solution.[24] One may hope that the competition between these entrepreneurs in social change will prevent the system from being launched on a disastrous course, and on this, "my hopes are as good as your fears," as Macbeath would put it.

Dynamic Monitoring of Social Policy in Canada

Any attempt to assess the contribution of social science research to social policy in Canada is made

particularly difficult by an almost total lack of reliable information on the very nature of the social science research being performed in this country. The Dominion Bureau of Statistics (1970) has recently prepared a general program to survey the social sciences, but it is still in a preliminary stage and there is little hope that the output of such an exercise will become available in the near future.

In the meantime, interested students have to rely on "guesstimates" even to ascertain the broad contours of the Canadian social science effort. As long as one does not derive a false impression of certainty from these quantitative benchmarks, they can prove very useful. For instance, we have presented in Table I the D.B.S. guesstimates submitted to the Organisation for Economic Cooperation and Development in 1970 about the structure of social science activities in Canada in 1967.

The reliability of these orders of magnitude does not have to be taken on faith. It can be very roughly verified by comparisons with the information stores in the Proceedings of the Senate Special Committee on Science Policy. We have attempted to reconstruct from data scattered in the Proceedings both a guesstimate of the intramural expenditures of government on social sciences ($37.5 million in Table I) and a guesstimate of the total current government expenditures on social science activities ($55.5 million in Table I. We chose these components because the data on government expenditures on social science activities is more readily available. From this exercise of verification spelled out in Tables 2 and 3, we obtain orders of magnitude of $37.98 million and $56.8 million respectively. This is enough to assure ourselves that we are in the right ball park.[25]

It is difficult to ascertain how much of the government expenditure on social science activities is for the purpose of monitoring the socio-economic guidance system of the government in Canada.

TABLE I

Structure of Current Expenditures on Social Science Activities (Mainly Research and Development) by Performer and Sources of Funds, Canada 1967 (in Millions of Dollars)

PERFORMER

Source of Funds	Government	Higher Education and Others	Business	Total
Government	37.5	16.0	2.0	55.5
Higher Education and Others		16.0		16.0
Business		2.0	8.0	10.0
Total	37.5	34.0	10.0	81.5

Source: D.B.S.—Special table—(1970).

a) This does not include social science research activities by industry. In France it has been estimated that these activities amount to one-quarter of the overall social science effort. If the same ratio is applied to Canada, we may then talk about some $100 million as the total current expenditure on social science activities.

b) This table does not segregate Research and Development from non-Research and Development expenditure.

TABLE 2

Structure of the Intra-Mural Expenditures on Social Science by Some Government Departments (in Millions of Dollars)

	1962-63	1963-64	1964-65	1965-66	1966-67	1967-68
Manpower					0.8	6.9
Transport				0.14	0.15	0.14
Economic Council			0.9	1.25	1.59	1.70
Bank of Canada	0.54	0.61	0.67	0.81	1.09	1.23
Labor					0.88	1.16
Indian Affairs and Northern Development	0.16	0.17	0.13	0.30	0.45	0.66
Central Mortgage and Housing Corporation (calendar year)	0.05		0.08	0.17	0.13	0.46
National Health and Welfare	0.46	0.46	0.56	0.67	0.85	1.00
Science Council and Secretariat			0.04	0.15	0.52	1.08
Dominion Bureau of Statistics	11.8	12.3	13.5	15.6	26.6	22.4
Agriculture (Economic)	0.91	0.99	0.98	0.98	1.04	1.25
Total	13.92	14.53	16.86	20.07	34.10	37.98

Source: See Table 3.

TABLE 3

A Downward-Biased "Guesstimate" of the Expenditures on Social Sciences by the Federal Government (in Millions of Dollars)

	(1) Canada Council[1]	(2) Task Force and Royal Commissions	(3) Mission-oriented Departments in Support of Universities	(4) Intra-Mural Expenditures on Social Science Activities[2]	(5) Total	(6) Adjusted Total[3]
1962-63				13.92	13.92	18.9
1963-64				14.53	14.53	19.5
1964-65	1.4			16.80	18.20	23.2
1965-66	3.1	3.1		20.07	26.27	28.2
1966-67	5.8	2.7	3.0	34.10	45.60	45.6
1967-68	11.5	3.3	4.1	37.98	56.88	56.8

Source: Senate Committee on Science Policy Proceedings

1. *Proceedings* . . . . p. 5224
2. Only a number of departments:

Manpower and Immigration, Proceedings	p. 4027
Transport, Proceedings	p. 2897
Economic Council, Proceedings	p. 3071
Bank of Canada, Proceedings	p. 3449
Labor, Proceedings	p. 3847
Central Mortgage and Housing Corporation	p. 4010
Indian Affairs and Northern Development, Proceedings	pp. 4337, 4370, 4403, 4449, 4455
National Health and Welfare, Proceedings	pp. 1052-53
Science Council and Science Secretariat, Proceedings	p. 3410 (French ed.)
Dominion Bureau of Statistics, Proceedings	p. 3590
Agriculture, Proceedings	p. 1185

3. The total is adjusted under the assumption that between 1962-63 and 1964-65, task force and Royal Commission expenditures amounted to $3 million per year; we have also made the further assumption that for the period from 1962-63 to 1965-66, mission-oriented expenditures in support of universities was at $2 million per year.

However, from an examination of the ratio of research and development to non-research and development expenditures in the physical sciences[26] and a critical look at the guesstimates of Table 1, we are led to infer that no less than $25 million were spent on the government intelligence function in social science research in 1967 and that a realistic estimate would probably be much higher. This lower bound estimate constituted 0.4 of 1 percent of Gross National Product in 1967.[27]

One may legitimately ask what this expense has accomplished. Is there any reason to believe that any dynamic monitoring in the sense that we discussed earlier has been generated as a result of these expenditures? If not, is there any reason to believe that the present social science research for evaluative purposes is at all useful in Canada? Is this sum sufficient to perform the important intelligence func-

tion we sketched above? The incredible ignorance that we have of the circumstances, of the significance, and of the effectiveness of social science research as an instrument to evaluate and to improve social policy is a shield behind which all possible positions can comfortably hide. Instances for and against the contention that social science research has effectively guided social policy can be quoted. But, on the whole, if asked whether one can ascribe responsibility for great successes and failures in Canadian social policy (understood to be dealing with the integrative system) to social science research, I would have to say that the answer is no.[28]

As I try to read the palimpsest of the Canadian social policy, I find very little which can be ascribed to social science research and even less that can be ascribed to the dynamic monitoring of previous policies. This is due to a number of factors: in part to the fact that too many Canadian social scientists have withdrawn to their ivory tower; in part to the fact that whenever they have jumped in, their expertise has too often proved irrelevant if not completely bogus; and, in part to the fact that, when the few who had the capacity and will to monitor the system and to improve the social technology have become active, this has often not triggered important social improvements because of failures in the action programs, political and social leadership, and in the functioning of institutions. I am not at this point ready to apportion the blame among these three families of causes.

However, I am willing to state unambiguously that social scientists would be ill-advised to ascribe all of the blame to the third family and to proceed lightly to pin the responsibility for such delays on the politicians alone. The effectiveness of social science research for social policy depends on the channels into which its results are plugged, and it is the responsibility of the social science research system to ensure that its output reaches the decision-maker, i.e., the person who manipulates the levers. It is not sufficient for the social scientist to translate the problem into a language of problem solution and to solve it: he must also translate his answers into a language of persuasion and ensure that it reaches the decision-maker. If this entails a new division of labor in the social sciences, and if we still have a serious lack of social engineers, this is a flaw in our social science apparatus.

Like any summary statement, my generalization is vulnerable. Some can argue that it is incorrect by reciting a few success stories; others have argued that, while it may be factually correct, it is a misleading conclusion derived from an insufficient appreciation of the tardiness of reasonable social science expenditures in Canada and of the need for a critical mass of research before it can become effective. It is not my intention to deny the few success stories nor to reject entirely the infant social science argument. However, I contend that it would be scandalous to continue to pump additional money into social science research in the name of improvement to social policy without insuring that it will indeed provide some help. To do more of the same may not be rational.

The solution lies, therefore, much more in a transformation of social science research to enable it to contribute to the evaluation and the improvement of social policy than in enlarged financial support. Science, social or not, "is not only an exciting adventure for scholars and a technique for economic expansion, it is also an essential part of the social organism and must continuously evolve under the influence of social conditions." (Dubos, 1970: 17)

The last decade has seen the emergence of a mood of experimentation which can only be a good omen. The issuing of White Papers and the setting up of special committees of the House of Commons or the Senate on certain aspects of social policy, the spreading practice of task-forcing, and the greater use of planned non-permanent institutions to deal with certain problem fields have all contributed to provide some interplay between the analysis and the problem formulation. They also have prevented the premature and / or permanent crystallization of institutions at a time when the problem field had been still insufficiently explored.[29]

But this willingness on the part of governments to experiment must be matched by a willingness on the part of the social scientists to participate. Before the government starts pouring additional money into social science research and hiring a herd of social scientists, both parties must agree on the broad outline of an interface between the social sciences and social policy. "A new deal must be negotiated between the politician and the social scientist." (Lamontagne, 1969: 4837)

As to the content of this new deal, it might be unwise for one individual to venture a guess. However, if one were to accept my diagnosis of section C, it would follow that any new deal must first entail the setting up of a forum where the guiding principles would be hammered out and the norms set. Second, this new deal would require some loci both within and without government where the dialectics administrator-researcher could follow its course.

There exists a diversity of models for the sort of forum suggested above. Arthur Stinson reviewed a number of them in his 1968 memo, and he even delineated the contour of a Canadian Institute for Social Development; one might follow Bertrand de Jouvenel (1964) and sketch a "forum prévisionnel"; another possibility in which I personally expressed interest a few years ago would be a Council of Social Values. In all cases, these tribunes would be future-oriented and they would contribute to this expansion of the "arts de conseil" that de Jouvenel predicted (de Jouvenel, 1964: 346) for the second half of the Twentieth Century.

As for the locus for the dialectics administrator-researcher inside and outside the public administration, it would have to be embodied in a system composed of different types of organization dealing with various types of problems at different levels in specialized settings. In 1964, the Council of the Tavistock Institute of Human Relations prepared a document which might helpfully serve as a frame of reference in designing the optimal social science research for Canada. This document spells out a useful classification scheme of the characteristics of the main types of research organizations and puts forward a plausible and feasible system of institutions which could be established over the next decades in Great Britain. This might constitute a useful point of departure for the elaboration of a Canadian mix of organizations likely best to meet our needs.

There already exist both within and outside of the government establishment a number of viable units with diverse vocations and most importantly not part of a coordinated network.[10] Their usefulness would be greatly enhanced by a grouping and balancing of the disciplines and activities into a set of only partially overlapping units covering the whole policy space. The matching of outside units with inside commandos should ensure that the policy multilogue can be effectively carried.

On the basis of this new deal between the social scientist and the social administrator and of the coordination of these loci for the policy multilogue around a sort of Council of Social Values, social scientists are bound to become more effectively present in the government operations. It will remain for them to prove their usefulness. My contention is that a dramatic re-tooling of social scientists is necessary for them to be useful and that the accompanying institutional attunements are bound to accelerate the process of rethreading, which is now progressing very slowly.

Once we have better social science data and a refurbished social science arsenal on many fronts, we might wish to produce an annual social report on the Canadian scene, one which would go much further than the premature one produced in the United States. (Olson, 1969; U.S. Department of Health, Education, and Welfare, 1969) This would generalize to the country as a whole "an orderly framework for our thought" on social policy. One might then be even adventurous enough to conceive of a Council of Social Advisors along the lines suggested by Bertram Gross.

Conclusion

Both social policy and social science research have evolved over the last decades and their interface is not as clearly understood as one would like. Social policy encompasses the whole integrative system and poses ill-structured problems to social scientists. If social science research has matured, its expertise still pertains almost exclusively to "established" rather than "emergent" situations. (Boguslaw, 1961) There is now a pressing need to bridge this gap. The old division of labor between goal and norm setting, control, evaluation, and innovation does not seem to fit present day policy-making processes. In the words of Senator Lamontagne, "a new deal must be negotiated between the politician and the social scientist." This new deal might allow the social scientist to indulge in heuristic system design on the basis only of some guiding principles of social action. Recognizing fully the very tentative nature of his hypothesis, the social scientist would require a strong feedback between his problem formulation and the analysis he is conducting. By a practical use of experimental utopia and simulation techniques, he could explore the lateral possibilities derivable from his original hypothesis, and examine their desirability.

This process-oriented analysis blurs the traditional difference between innovation and control-monitoring, but it appears to be the only effective response to the accelerated change which identifies our temporary society. Up to now, in Canada as well as in France (Crozier, 1970), the few dozen research social scientists and the few hundred instructors dabbling in social science research have had little impact on the conduct of social policy. This is largely due to a maladaptation of social scientists. In a slow-motion society, one could study the functioning of institutions in a strict positivistic manner. In a temporary society, the very mode of reasoning of the social scientists has to be altered, and priority must be given to experimentation. A few preliminary successful experiments in Canada have led us to believe that the time is ripe for what Crozier calls (*Ibid*: 17)

"les premiers essais expérimentaux au niveau d'organisations ou d'institutions complexes." We have sketched some of the elements which must be considered in tackling the social policy system in this manner.

Footnotes

*Revised version of the paper presented at the Seminar on Social Science Research and Social Policy. I would like to acknowledge the assistance of Gary Trolley on this project.

[1]"The low-structure organization shifts constantly. It organizes itself around objectives rather than elaborate charts, reporting relationships and job descriptions which specify formal channels of communication and strict measures of accountability. Most important of all, its members are free—and they know they are free—to function as their jobs require." (Hughes, 1965: 52)

[2]"To be plain, the expertise that the transport economist can bring to bear on traffic problems in town and city is largely bogus. I do not allege that he is consciously perpetrating a fraud. Like too many in the social sciences he is impatient to display his scientific credentials, anxious to be seen measuring, that is, even where it entails, as it so manifestly does in this case, measuring much the smaller part of the problem for lack of a method to capture the larger." (Mishan, 1967: 97)

[3]This is not to deny that the social sciences have made important breakthroughs over the last century. One could indeed point to major achievements in the Twentieth Century. For instance, see K. W Deutsch, S. Platt, D. Senghass (1971) who provide an analysis of 62 such advances since 1900. (Dr. K. Krotki kindly brought this paper to my attention.) The issue here has to do with the usefulness and the effective use of such social science achievements to solve social problems and to design social policy.

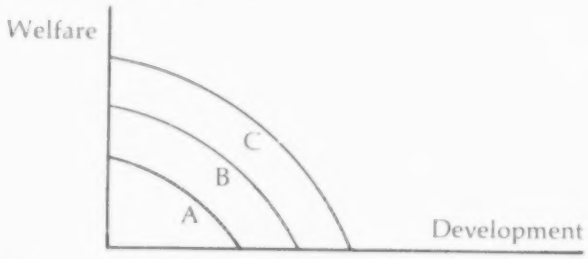
[4]For a simple and illuminating discussion of the stages of scientific activity, see Kuhn, 1963, Ch. 3.

[5]"Le progrès de la connaissance consiste pour une part à écarter les analogies superficielles et à découvrir des parentés profondes, moins visibles peut-être, mais plus importantes et significatives." (Caillois, 1960:9)

". . . déterminer des correspondances souterraines, invisibles, unimaginables pour le profane." (*Ibid.* 17) See also Kelly, 1963.

[6]Eric Trist develops those ideas very clearly in the *Proceedings of the Senate Committee on Science Policy*, 1st Session, 28th Parliament, 1968-69, No. 39, March 29, 1969, pp. 4794-4820. This vocabulary has now passed into the government's language—see the *White Paper on Unemployment Insurance*, Ottawa, 1970.

[7]See *A Comprehensive Statement on Social Welfare for Canada*, Canadian Welfare Council, May, 1967; also Manning, 1967. A review of the structure of benefits in different countries in the United Nations Yearbook shows that as the resources available increase (i.e., as the countries get richer cross-sectionally) the mix of benefits shifts toward a relatively more development-intensive policy, although both components are higher obviously in richer than in poorer countries. The direction of social policy seems to follow an A B C path.



[8]Titmuss's eight points (1968: 22-23) read as follows:

"1. The analysis description of policy formation and its consequences, intended and unintended.

2. The study of structure, function, organization, planning and administrative process of institutions and agencies, historical and comparative.

3. The study of social needs and of the problems of access to utilization and patterns of outcome of services, transactions and transfers.

4. The analysis of the nature, attributes and distribution of social costs and diswelfares.

5. The analysis of distributive and allocative patterns in command-over-resources-through-time and the particular impact of the social services.

6. The study of the roles and functions of elected representatives, professional workers, administrators and interest groups in operation and performance of social welfare institutions.

7. The study of the social rights of the citizen as contributor, participant and user of social services.

8. The study of the role of government (local and central) as an allocator of values and of rights to social property as expressed through social and administrative law and other rule-making channels."

[8]A few words may be in order on the exact nature of these four categories of issues. (1) *Goal setting* raises the problem of the organization's objectives. There is in this context an optimum degree of secrecy. "The art of bluff and deception with respect to goals is part of the art of administration." But the real reason for embarrassment here is, in Gross's terms (1965), "the lack of well-developed language of organizational purposefulness." (2) *Control* has to do with getting the work done. It is more in line with operations analysis. To the extent that an objective function can be ground out by the goal-setters a "rational" strategy can be produced. This rationality can be either technical or administrative. It is fair to say that up to now the process of control has been mainly reduced to the manipulation of so-called controlled variables so as to maximize some specified function subject to well-defined constraints. (3) While control refers mainly to problems of functioning of an organization, *innovation* refers to the system design and to the change in the organizational structure and in the administrative arrangements. (4) *Intelligence* refers to the "problem of gathering, processing, interpreting and communicating the technical and political information needed in the decision-making process." For a review of alternative approaches to praxeology, see Kotarbinski, 1965 and Kaufman, 1968.

[9]For an example of the traditional literature, see Lerner and Lasswell, 1951. As to the more recent directions, the importance of intelligence, the shift from goal-setting to norm-setting and holding and policy-making as a learning process, see Vickers, 1965.

[10]A simple sketch of these polar approaches has been presented by Gordon, 1970 (Ch. 3). He titles this chapter very aptly—"Blueprints vs. Little Steps."

[11]It is always possible to force the problem into a mould. The result is that a set of goals "jêtes là au hasard" and whose signification is hardly well understood become the guidance system for ascertaining the performance of the system. The rational pursuit of arbitrarily set goals is not an attractive alternative to a control mechanism which, like yesterday's Treasury Board, was performing only an accounting job.

[12]Ansoff (1960) specifies the adjustments to the method of analysis which are necessary when the problem is ill-structured. We shall refer to this very important paper later on.

[13]It may be characterized in Ansoff's words as follows (1960: 230):

(a) It is process- rather than goal-oriented. Rather than comparing the likely outcomes of various alternative strategies, it emphasizes comparison of their characteristics (thus inferring likely outcomes);

(b) It utilizes qualitative judgments, embedded within the problem structure;

(c) It maintains continuous feedback between analysis and problem formulation, thus constructing a self-correcting theory of the business, as it proceeds. (The model of the whole cannot be made explicit in advance of the solution, but only in terms of symbolic relations between variables);

(d) It treats several (often mutually exclusive) business objectives simultaneously until a set of dominant alternatives is found;

(e) It divides strategic business problems into (1) external (concerned with the firm's opportunities in its environment) and (2) internal (concerned with the firm's strengths, and weaknesses); it divides each such problem further into its quantitative and qualitative sub-problems;

(f) It arrives at a family of solutions by successively narrowing the field of alternatives by means of eliminating criteria;

(g) It arrives at a final solution by applying some weighting procedure to the residual set of alternatives;

(h) It emphasizes comprehensiveness and validity by abstracting the real-life situation (rather than precision and mathematical formulation);

(i) It is neither well-structured (as the operations research approach), nor unstructured (as the heuristic approach), but rather ill-structured. It is thus methodical without being mathematically rigorous.

[15]The self-organizing system is genuinely a learning system and as Sir Geoffrey Vickers puts it "the future of our society depends on the speed with which it can learn." (*op. cit.* p. 233) Much of this new function of organizational intelligence lies in the capacity it has to speed up the process. The idea of "experimental intelligence" and of "socially organized intelligence" was already clearly spelled out by John Dewey in 1935. Social scientists, however, abandoned the path of social experimentation after the 1930s. Consequently, the interest of the policy-maker diminished. According to Fred Harris (1970:3), commenting on the U.S. situation, "in 1938 the social sciences received 24 percent of total govern-

ment expenditures for research. By 1950 this had diminished to about 8 percent, and for the last ten years the social sciences have been allocated between 3½ and 5 percent of federal funds for research."

[16]Social policy becomes the nexus of social science and social ethics. Since the norm setting replaces in some way the old goal setting, social scientists must recognize their increased responsibility. However, the paucity of social-ethical reflections is such that much of this exercise is solely guided by a sort of "technological rationality" emerging from the modeling exercises themselves. This is the basis of the sharp critique proposed by Winter, 1966. For a primer on the ethics of large scale systems, see Churchman, 1968, Part III.

[17]What is involved here is a dialectical process which eliminates the usual "chronological point at which the formulation ends and the data-gathering and analysis," and policy-making (we could add), begin. (Ansoff, *op. cit.*, p. 242)

[18]Lefebvre, 1968: 121. This refers to a form of reasoning which is in full accord with the quasi-analytical methods required by the ill-structured problems of social policy. Moreover, the idea has credibility because it has been supported by both theoretical and applied work. On the first point, see Mandelbrot (1953: 43); on the second front, see *Die Neue Stadt. Eine Studie für das Furttal.*(Zurich, no date),in which a group of social scientists and architects have applied the method to urban problems is possibly the most ill-structured of the problems of social policy. Lefebvre discussed this study at length in *Du rural à l'urbain.* (Paris, 1970, pp. 129-140).

[19]It is interesting to note that although the label is new, the process involved in experimental utopia is rather well rooted in the literature of the 1930s. A careful reading of John Dewey's *Liberalism and Social Action* (1935) reveals that "the method of experimental intelligence" and "social experimentation" in social policy had already been broadly sketched by the 1930s.

[20]There is already an extensive literature on the subject. Over the wide range of viewpoints on it, we have picked three: de Jouvenel, 1964: 361-ff; Barton, 1970; Naylor, 1971.

[21]For an examination of the possible way in which one could formalize the process of development and the self-steering of a system, see Lange, 1965.

[22]Boguslaw, *op. cit.* Note especially that *heuristic* does not have in this context its usual dictionary meaning. The word is taken here "to denote any principle or device that contributes to the reduction in the average search to solution." (p. 15)

[23]For some reason, this approach (although I explicitly regard it as a second best) has been interpreted by some readers of this paper as the favored approach, the one which would obviously give maximum leverage to the expert who would be able to get the ear of the prince. Indeed, while it may be easier and desirable for an entrepreneur in social change to operate this way, it may be undesirable from the point of view of the system. But as a second-best procedure, it should not be discarded. On the other hand, "outside advisors" are not and cannot be regarded by healthy government organizations as anything more than a second-best answer to their problem of dynamic adaptation. See DesRoches, 1970.

[24]On the new role of social scientists as entrepreneurs in social change, see Breton and Breton, 1969; also Davis and North, 1970. See also Bennis, 1964.

It might be worth adding that in some sense this second-best approach has often proved a better way to plug social science research results of interest into social policy than the usual commission studies. Some of the comments of Dr. Otto Larsen on the effectiveness of the U.S. Commission on Obscenity and Pornography would tend to support this view. Even if we were to be able to bring about a more open instrumentation for smooth social change and social development, it will be necessary to ensure that the dialectics researcher-manager is as close as in the case of the operating unit approach.

[25]The guesstimates produced by the Dominion Bureau of Statistics (Table 1) are referring to current research and development expenditures in the social sciences plus some component of the non-research and development segment. Since no segregation of research and development and non-research and development exists in the social sciences in the present data, this is the only possible procedure. Our own estimates (Tables 2 and 3) also ignore this segregation but there is no way to ensure that the mix of research and development and non-research and development is the same as in the Dominion Bureau's estimates. Indeed, there are some reasons to believe that we may include more non-research and development expenditures than the Bureau's estimates: for instance, we include most of D.B.S. expenditures.

This would explain that with a reduced basis including only some of the departments at the federal level and ignoring the provincial government activities (which are small anyway) we can get the same order of magnitude that D.B.S. got. This also

means that both sets of estimates may be regarded as lower bound guesstimates: it is likely therefore that total current expenditures on social science activities are well above $100 million—the figure we tentatively ventured in the footnote to Table 1.

[26]In order to obtain an estimate of the monies allocated in Canada to the intelligence function in the social sciences, I have postulated that this function would be akin to the non-research and development component.

[27]We have chosen not to break down this total into the economic policy-oriented and the social policy-oriented expenditures. If asked to provide a rule-of-thumb for such separation between the monitoring of the exchange system and of the integrative system, we might venture to say that one-quarter of the sum might be allocated to the latter. This is, however, strictly an impressionistic if educated guess.

[28]David W. Slater has answered in the same manner to an analagous question posed about economic research and economic policy (1967).

Since it is well recognized that both the economic theory and the economic information system are more sophisticated than the theories and information apparati in the other social sciences, Slater's conclusion is bound to hold *a fortiori* for the other social sciences. The question, I repeat, is not to deny that there is some social science knowledge that could be used, but to reiterate that, in the same way that the universities did not participate in the Renaissance, the social scientists have been and are of little use to social activists and social administrators in the designing of social policy. The problem was stated unambiguously by Martin Luther King (1968), "One reason some advances were made in the South during the past decade was the discovery by Northern whites of the brutal fact of Southern segregated life. It was the Negro who educated the nation by dramatizing the evils through non-violent protest. The social scientist played little or no role in disclosing truth."

[29]Neither the White Papers, nor the committees, nor the task forces, nor the creation of "temporary" departments (Department of Industry, for instance) are really a panacea. It constitutes, however, an opening of the dialectics researcher-manager to new possibilities. Social scientists have often not reacted at all to the challenge of the policy-maker in his White Papers; too often, they have used the task forces as a vehicle to pursue their own (sometimes irrelevant) researches rather than as a chance to explore anew a problem field and to provide the policy-maker with a new frame of reference; they have also used the new-born more impatient departments as more naive providers of research monies without realizing what their precise vocation was. No autopsy of those experiments has been performed yet.

The idea of a Council of Social Values (of a restricted sort) was first put forward by Hansen, (1960:91). I developed the idea a bit in a paper on "The Economic Council as Phoenix." (1968).

[30]A review of some of the existing institutions from the Canadian Council on Urban and Regional Research to the Canadian Council on Rural Development via the Institute for the Quantitative Analysis of Social and Economic Policy of the University of Toronto has been presented by Stinson (1968). Some of the very recent experiences in Ottawa (Treasury Board, Department of Finance, Unemployment Insurance Commission, Department of Housing and Urban Development), demonstrate that the politicians are more receptive to setting up a fifth column of social scientists close to policy-making.

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